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THE PSYCHOLOGICAL REVIEW

STATISTICAL ANALYSES OF THE MEMBERS AND ASSOCIATES OF THE AMERICAN PSYCHOLOGICAL ASSOCIATION, INC. IN 1928

A CROSS SECTION OF AMERICAN PROFESSIONAL PSYCHOLOGY

BY SAMUEL W. FERNBERGER

University of Pennsylvania

Eight years ago, Boring and the writer[1] statistically analyzed the membership of the American Psychological Association as it appeared in 1920. Since that time the numbers in the Association have increased enormously and this increase has especially been in the newly organized group of Associates whose qualifications for election are lower than those for Membership. With this increase in members and with the addition of the associate grade, the present total in the Association would seem to be a truer picture of American professional psychology than it was in 1920. Throughout the present paper, the words psychology and psychologist designate only professional psychologists of standing and cannot take account of the large group of individuals throughout the country calling themselves psychologists—few of whom possess the qualifications for election to membership, or even associateship, in the Association. It is the writer's belief that only a few individuals possessing

[1] Boring, E. G., 'Statistics of the American Psychological Association in 1920,' PSYCHOL. BULL., 1920, 17, 271–278.

Boring, E. G., 'Geographical Distribution of Psychologists,' PSYCHOL. BULL., 1920, 17, 64.

Fernberger, S. W., 'Further Statistics of the American Psychological Association,' PSYCHOL. BULL., 1921, 18, 569–572.

the qualifications for membership have failed to join and maintain their membership in the Association. This statement is certainly not true for individuals qualified for associateship. In the present statistical analysis [2] no distinction is made between members and associates.

Geographical Distribution

The following list indicates the geographical distribution of psychologists by states for the United States and Canada,

New York	123	Oklahoma	4
Illinois	62	Utah	4
Massachusetts	61	Maine	3
Pennsylvania	54	Nebraska	3
Ohio	53	New Hampshire	3
California	46	Rhode Island	3
Connecticut	27	South Dakota	3
Michigan	21	Vermont	3
Minnesota	20	Wyoming	3
New Jersey	19	Hawaii	3
Iowa	18	Idaho	2
Maryland	16	Oregon	2
Wisconsin	16	West Virginia	2
District of Columbia	14	Alabama	1
North Carolina	11	Arizona	1
Kansas	10	Mississippi	1
Missouri	9	Montana	1
Washington	8	New Mexico	1
Colorado	7	South Carolina	1
Indiana	7	Arkansas	0
Tennessee	7	Delaware	0
Virginia	6	Nevada	0
Florida	5		
Georgia	5	*Foreign*	
Kentucky	5	Canada	7
Texas	5	Austria	1
Louisiana	4	Germany	1
North Dakota	4		

From such a list it is possible to calculate the center of population of psychologists for the United States. This may be calculated by either of two methods: (1) the calculation of the median, or (2) the calculation of the average. The calculation by the median method brings the center of

[2] The material for this statistical analysis is taken from the 1928 Year-Book of the American Psychological Association.

population close to Cleveland, Ohio. The calculation by the average method places the center of population about 40 miles due southeast of the northwest corner of Ohio. This is about the center of the triangle defined by Toledo, O., Lima, O. and Fort Wayne, Ind. It is not near any important psychological center. Boring found this point in 1920 to be latitude 40° 45′ and longitude 84° 20′. We find it 8 years later to be 40° 55′ and longitude 84° 20′, which indicates surprisingly little change.

The location of this point has an important bearing on the proper places of meeting of the Association, provided convenience and average distance of travel are alone to be considered. For this comparison, it is undoubtedly more fair to consider the average center of population rather than the median. If the Association should decide that they should go no further east than Boston (certainly the most easternly point to be considered) then the Association should not meet further west than Minnesota, Missouri and Iowa. They should, however, meet as far south as South Carolina and northern Georgia and as far southwest as Tennessee. A meeting outside of these limits must be determined on grounds which outweigh distance of travel. This is an added motive for the development of the Western Section of the Association, inasmuch as these members cannot be expected to travel east with any great frequency, any more than they could expect a meeting of the general Association within their district.

But, if the Pacific Coast psychologists are eliminated from consideration, then the center of population by the average method would fall in northwestern Pennsylvania. Under these conditions, we should not meet further west than Wisconsin and Illinois when Boston is considered the most easternly point of meeting.

Academic Antecedents

The following list gives the place or institution for obtaining the Ph.D. degree of the 616 members and associates who report both the degree and the place from which it was obtained.

Columbia	135	Boston	2
Chicago	80	Nebraska	2
Harvard	56	Cincinnati	1
Clark	53	Hartford Seminary	1
Cornell	34	Illinois Wesleyan	1
Iowa	33	Indiana	1
Hopkins	30	Miami	1
Pennsylvania	30	Missouri	1
Yale	23	Northwestern	1
Stanford	21	Union	1
Michigan	11	Washington	1
Ohio State	8		
Illinois	7	*Foreign*	
Minnesota	7	Leipzig	9
Princeton	7	Berlin	5
George Washington	5	Zurich	3
Peabody	5	Bonn	2
Radcliffe	5	Freiburg	2
Bryn Mawr	4	Wuerzburg	2
California	4	Glasgow	1
Carnegie	4	Halle	1
Wisconsin	4	Heidelberg	1
Catholic	3	Jena	1
New York Univ.	3	Munich	1
Brown	2	Stockholm	1

Of these, 29 degrees were given by foreign institutions (5% of the total). Of the remaining 587 individuals, a total of 324 or 53% were given by 4 institutions (Columbia, Chicago, Harvard and Clark). And a total of 495 or 80% of the American degrees were given by 10 institutions (the four indicated above plus Cornell, Iowa, Hopkins, Pennsylvania, Yale and Stanford). Only 92 (or 15% of the American degrees) were given by the other 26 institutions on the list.

Since 1920, the position of the first five institutions in the list has remained unchanged. Iowa has advanced from ninth to sixth place. Stanford has advanced from eleventh to tenth place. Only 70 degrees were obtained west of the Mississippi. In 1920 some 94.7% of the degrees were obtained east of the Mississippi as against 88% in 1928. The rise of Stanford as a degree-giving institution has gone a long way toward this reduction in percentage. The number of foreign Ph.D.'s in the Association has declined from 36 in 1920 to 29 in 1928.

As in 1920, the situation with regard to where psychologists received their first inspiration for psychology, as surmised from the place of taking the first degree, is very different. The following list gives the number of colleges and universities for such lower degrees.

Harvard	23	Ohio State	13
Indiana	22	Pennsylvania	13
Columbia	21	California	12
Nebraska	21	Iowa	12
Chicago	19	Washington	12
Yale	18	Northwestern	11
Michigan	16	Amherst	10
Vassar	15	Cornell	10
Illinois	14	Clark	9
Stanford	14		

8 each: Dartmouth, Missouri, Smith, Wisconsin.

7 each: Colorado, Princeton.

6 each: Barnard, Hopkins, Kansas, Oberlin, Ohio Univ., Oregon, Texas, Toronto, Wesleyan.

5 each: Bryn Mawr, Holyoke, Minnesota.

4 each: Brown, C.C.N.Y., Gettysburg, Lawrence, Ohio Wesleyan, Rochester, Utah, Vanderbilt, Wellesley, West Virginia.

3 each: Baker, Bethany, Boston, Bowdoin, Brigham Young, Bucknell, Cincinnati, Cornell College, De Pauw, Goucher, Hamilton, Haverford, Iowa State T.C., Juniata, Lafayette, McGill, Muhlenberg, Oklahoma, Pittsburg, Randolph-Macon, Richmond, Springfield, Wake Forest, Williams, Wyoming.

2 each: Arkansas, Beloit, Campbell, Central, Denver, Drake, Dubuque, Earlham, Emory, George Washington, Hiram, LaGrange, Leander, Clark, Lebanon, Marietta, Mass. Inst. Tech., Mercer, Mills, Missouri Valley, New Hampshire, South Dakota, Tufts, Tulane, Union, Ursinus, Virginia, Wells.

1 each: Adrian, Alberta, Alfred, Allegheny, Ames, Antioch, Arkansas Coll., Augustana, Baldwin Wallace, Bates, Bridgewater, Case, Chattanooga, Coe, Colgate, Colorado State T.C., Conn. Wesleyan, Cotner, Cumberland, Denison, Des Moines, Dickinson, Ellsworth, Elmira, Evansville, Florida, Florida State Coll. for Women, Franklin & Marshall, Furman, Geneva, Grand Island, Greenville, Grinnell, Gustavus Adolphus, Hamline, Hedding, Hobart, Holy Cross, Illinois Coll., Illinois Wesleyan, Johnson, Judson, Kansas Wesleyan, Knox, Lombard, Mass. Agri. Coll., McMaster, McPherson, Michigan State, Missouri State, Morningside, Muskingum, Nashville, Nebraska State, New York Univ., North Carolina, Occidental, Olivet, Ouachita, Pacific, Park, Parsons, Penna. State, Radcliffe, South Carolina, S.E. Missouri State T.C., Southern California, Southwestern, St. Bonaventure, St. Lawrence, St. Olaf, Swarthmore, Taylor, Temple, Transylvania, Trinity (Conn.), Trinity (N.C.), Valparaiso, Vermont, Wabash, Washburn, Washington & Lee, Washington State, Western Reserve, Westminster, William & Mary, Wilson, Wittenberg, Wooster, York.

Foreign Inspiration

2 each: Dalhousie, Edinburgh, Oxford.

1 each: Cambridge, Cape Town, Glasgow, Neuchatel, New Zealand, Olso, Peking, St. Andrews, Stockholm, Zürich.

There are 190 American and 13 foreign institutions in this 'inspiration' list. Hence the four institutions responsible for 53% of the Ph.D.'s inspired less than 12% of the Association membership for work in psychology; and the first 10 institutions, responsible for 80% of the Ph.D.'s inspired less than 24% for this work.

Another curious fact is that Indiana and Nebraska with only one Ph.D. each, rank second and fourth in the inspiration list, while Vassar, without any Ph.D. ranks eighth.

We can still conclude, as we did in 1920: "These facts would seem to indicate that, although the interest is widespread in this country and although one finds psychologists everywhere who are capable of arousing a lasting interest in the work among students, still the number of laboratories equipped to give adequate post-graduate work leading to the Ph.D. degree, is very small."

Indeed only seven more American Ph.D. granting universities have been added to the list since 1920 (Peabody, Carnegie, Cincinnati, Hartford Seminary, Miami, Northwestern and Washington). Of these the last five are responsible for only 1 Ph.D. each. Amherst has been lost from the Ph.D. list since 1920.

Problem of Inbreeding in Psychological Departments

In recent surveys of particular American colleges and of American college education in general much has been made of the inbreeding of the faculties of the different departments. The author does not wish to indicate his own opinion as to whether inbreeding is a good or a bad thing. Those interested in higher education seem to believe that it is an evil. Apparently much may be said on both sides. The facts with regard to the departments of psychology are of interest however.

The following list gives the number of individuals who

have stayed on or returned to the place from which they received their final degree. In this list only psychologists are included: those indicating neurology, education and the like as their major interest having been eliminated. The size of the total department appears in parentheses after the number inbred for those cases in which there are a relatively large number. The second column, in parentheses, indicates the total number of individuals in the departments and the last column gives the percentage of inbreeding.

Columbia	17	(29)	59%
Chicago	8	(15)	53
Iowa	7	(12)	58
Pennsylvania	7	(8)	88
Harvard	5	(9)	56
Stanford	4	(11)	36
Michigan	3	(12)	25

2 each: Catholic, Cornell, Hopkins, Illinois, Princeton.

1 each: Bryn Mawr, California, Cincinnati, Dartmouth, Florida, George Washington, Hartford Seminary, Hawaii, Minnesota, New York Univ., Northwestern, Ohio State, Peabody, Toronto, Wisconsin, Yale.

Thus six of the ten departments responsible for the great number of Ph.D.'s show various degrees of inbreeding. When reduced to a percentage basis, the rank order is: Pennsylvania, Columbia, Iowa, Harvard, Chicago, Stanford, and Michigan. Those other departments in the first ten of the list for granting Ph.D.'s, who show little inbreeding are Clark (a recently reconstructed department); Cornell (a relatively small department and actually 50% inbred); Hopkins (another relatively small department and actually 50% inbred) and Yale (surprisingly little inbred but with the Institute recently added).

It would seem, therefore, that the charge of inbreeding may be justly applied to the important laboratories of psychology in America. It seems to the writer, however, that most of these laboratories are curiously free from the rigid systematic bias and points of view that such inbreeding is supposed to imply.

Another aspect of this problem is a consideration of those individuals who started work at one institution, went elsewhere for their graduate work and then returned to teach at the first college or university. A list of these follows:

Indiana	3
California	2
Northwestern	2
Oberlin	2
Vassar	2

1 each: Brigham Young, Cornell, Denver, Dickinson, Emory, Geneva, Hobart, Lawrence, McGill, Ohio State, Pennsylvania, Pittsburgh, Princeton, South Dakota, Toronto, Tufts, Utah, Vanderbilt, Washington & Lee, Wabash, Wellesley, West Virginia, Wooster, Wyoming.

Again only academic psychologists are considered. The impressive thing about this list is the number of smaller colleges who apparently have sent promising young men out to properly prepare themselves to return to teach psychology.

Academic and Non-Academic Positions

An analysis of the positions held by members and associates of the Association is given below:

Academic Position in Psychology in College or University

Professor	198	Assistant Editor	1
Associate Professor	63	Home Study Department	2
Assistant Professor	61	Graduate Student	1
Instructor	32	Once Psychologist now retired	3
Assistant	1	College and University Presidents	10
Associate	6	Research in psychology but no academic position	2
Research Associate	3	National Research Fellows	5
Lecturer	4		

Academic Position in Psychology Lower than Colleges (Normal Schools, etc.) 32

No Academic Position

Clinical practice and clinical research	104
Industrial practice and research	19
No position in psychology	52

Academic Position but Not in Psychology

Education	39	Psychiatry	4
Philosophy	19	Neurology	2
Administration	5	Zoology	2
Medical School	5	Physiology	2
Sociology	4		

1 each: Otology, Nervous Diseases, Religious Education, Social Ethics, Pharmacology, Business Research, Advertising, Physics, Bible History, Speech, German, Anthropology.

From this list it is evident that about 57% of the individuals in the Association are holding academic positions in colleges and universities in psychology. Under 5% are in academic positions lower than colleges (Normal Schools, etc.).

About 15% are in academic positions but not in psychology and, of this 15%, over 8% are in education or philosophy. Nearly 18% are in straight clinical or industrial practice or research and finally over 7% have no professional position whatsoever. Hence, in spite of the efforts of the Association to keep itself 'pure,' only 80% of the membership and associateship are doing things that are strictly psychological in character.

Subjects of Instruction

In the consideration of the subjects of instruction, we have analyzed the data statistically for men and women separately and for different districts of the country. Our geographical analysis follows that of Boring with the following minor changes: We have here added six cases from eastern Canada to the New England totals and, to the Pacific Coast totals we have added one individual from British Columbia and three from Hawaii. The geographical sections, then, are those of the United States Census with the additions noted above: *New England:* Me., N. H., Vt., Mass., R. I., Ct., and Eastern Canada; *Middle Atlantic:* N. Y., N. J., Pa.; *Northeast Central:* Mich., Wis., Ohio, Ind., Ill.; *Northwest Central:* Minn., Iowa, Mo., N. Dak., S. Dak., Nebr., Kans.;

Table I

Subjects of Instruction (Men)

	Psychology	Applied Psychology	Education	Philosophy	Miscellaneous	No Instruction	Total
New England and Eastern Canada	34.9	21.4	4.2	4.5	6.3	14	85
Middle Atlantic	62.2	36.9	9.5	4.5	8.1	23	144
N.E. Central	46.1	28.2	6.7	5.0	5.6	19	111
N.W. Central	19.7	18.9	3.6	3.8	4.1	5	55
South Atlantic	18.6	8.2	1.5	1.5	4.2	8	42
S.E. Central	7.0	3.5	0.3	0.7	0.5	—	12
S.W. Central	6.0	4.2	—	0.3	0.5	—	12
Mountain	4.2	5.8	1.5	1.0	0.5	1	14
Pacific, Brit. Col. and Hawaii	17.9	16.5	1.5	1.3	1.7	1	40
Total	216.5	143.6	28.7	22.6	31.1	71	515

TABLE II

SUBJECTS OF INSTRUCTION (WOMEN)

	Psychology	Applied Psychology	Education	Philosophy	Miscellaneous	No Instruction	Total
New England and Eastern Canada	5.2	3.8	1.5	0.5	—	10	21
Middle Atlantic	6.0	17.0	1.5	1.5	—	24	50
N.E. Central	10.3	13.3	1.8	0.5	1.0	19	46
N.W. Central	0.8	6.2	—	—	—	5	12
South Atlantic	6.7	3.3	1.2	0.5	0.3	3	15
S.E. Central	1.0	—	—	—	—	1	2
S.W. Central	0.2	0.5	0.3	—	—	1	2
Mountain	1.2	0.9	—	0.7	0.2	—	3
Pacific, Brit. Col. and Hawaii	4.8	10.7	0.3	—	—	6	22
Total	36.3	55.7	6.6	3.7	1.5	69	173

TABLE III

SUBJECTS OF INSTRUCTION (MEN AND WOMEN)

	Psychology	Applied Psychology	Education	Philosophy	Miscellaneous	No Instruction	Total	Psychologists per 1,000,000 Population
New England and Eastern Canada	40.1	25.2	5.7	5.0	6.0	24	106	13.4
Middle Atlantic	68.2	53.9	11.0	6.0	8.1	47	194	7.7
N.E. Central	56.4	41.5	8.5	5.5	6.6	38	157	6.4
N.W. Central	20.5	25.1	3.6	3.6	4.1	10	67	5.3
South Atlantic	25.3	11.5	2.7	2.0	4.5	11	57	3.6
S.E. Central	8.0	3.5	0.3	0.7	0.5	1	14	1.5
S.W. Central	6.2	4.7	0.2	0.3	0.5	1	14	1.2
Mountain	5.4	6.7	1.5	1.7	0.7	1	17	4.2
Pacific, Brit. Col. and Hawaii	22.8	27.3	1.9	1.3	1.7	7	62	8.6
Total	252.8	199.3	35.3	26.1	32.6	140	688	5.9

South Atlantic: Del., Md., D. C., W. Va., Va., N. C., S. C., Ga., Fla.; *Southeast Central:* Ky., Tenn., Miss., Ala.; *Southwest Central:* Ark., La., Okla., Texas; *Mountain:* Mont., Wyo., Idaho, Colo., Utah, Nev., N. Mex., Ariz.; *Pacific:* Wash., Oregon, Calif., Brit. Col., Hawaii.

Tables I.–III. show, by this geographical distribution, the number of individuals giving instruction in the various sub-

jects listed. Table I. contains the data for 515 men; Table II. for 173 women and Table III. for the combined 688 men and women. Individuals in Europe, several who have recently died and several cases for whom the data in the Year-Book was inadequate were not considered in the tabulation. A person reporting more than one subject of instruction has been fractionated equally among the different columns.

The term *Psychology* includes general, experimental, theoretical and all of the special psychologies except applied. *Applied Psychology* includes all forms of application including educational psychology. *Education* includes pedagogy, experimental education, hygiene, school administration, etc. *Philosophy* also includes logic, ethics, etc. *Miscellaneous* includes all other subjects—largely statistics and æsthetics. *No Instruction* covers cases where no report was made in the Year-Book of subject of instruction.

A comparison of Table I. with the corresponding table in Boring's article of 8 years ago, indicates that the totals for *psychology* have remained almost identically the same—when one considers the increase in membership. Relatively, however, there has been a considerable increase in *applied psychology*. In 1920 only 12% of the men reported this as a subject of instruction and only 8 years later this percentage has more than doubled to 28%. This increase has been especially noticeable in the New England, Northeast Central and Pacific districts but it is evident throughout the country. On the other hand, there have been actual (as well as relative, of course) decreases in the number of men reporting *education* and *philosophy*.[3]

The situation is slightly different for women. While in 1920 only 7% of the women reported *applied psychology* as a subject of instruction, now this is reported by 32%—an increase which is relatively much greater than in the case of the men. For *psychology*, the women have relatively lost

[3] It should be noted that in 1920 the American Psychological Association still included quite a number of charter members whose interests lay wholly in philosophy but who had continued their membership after the founding of the American Philosophical Association. Most of these have since resigned, which exaggerates the apparent decrease in instruction in philosophy by psychologists.

ground—21% reporting this in 1928 as against 45% in 1920. For this subject of instruction the men retained their relative position. For *education* and *philosophy* the women also have had an absolute reduction.

Table III. and the similar table in Boring's paper, containing the subjects of instruction for men and women combined, give the total picture of instruction in academic psychology in America. A study of these tables definitely shows a trend away from laboratory psychology (from 43 to 37%) and a very marked increase of interest in applied psychology (from 11 to 29% of the total membership). With the absolute decrease of instruction in philosophy and education, there seems to be a trend of development of 'psychological consciousness' and psychological specialization divorced from these subjects.

The last column of Table III. gives the number of psychologists per 1,000,000 of population for the various geographical districts. The population figures are the estimates for 1928 made by the United States Census Bureau by states. For these columns the Canadian and Hawaiian members are eliminated. The relative order of districts remains relatively the same as it was in 1920. The notable change is the Pacific region which goes from sixth to second place. Although the Middle Atlantic and Northeast Central districts have actually more psychologists than any other, relatively New England clearly leads in the percentage of psychologists in the population. In this respect the Southeast Central and Southwest Central districts are notably weak.

It is of interest to note that, in spite of the rapid growth of population in the United States during the last 8 years, the increase of the number of psychologists has been relatively greater. In 1920 there were only 3.5 professional psychologists per 1,000,000 population and in 1928 this has been increased to 5.9. This increase has been general in all of the nine geographical units of the country.

TABLE IV

FIELDS OF RESEARCH (MEN)

	Psychology																				
	General	Abnormal	Psychopath.	Applied	Tests	Industrial	Clinical and Vocational	Educational	Animal	Social and Religious	Experimental	Genetic and Child	Theoretical	Philosophy	Aesthetics	Education	Physiology and Neurology	Statistics	No Research	Total
New England and E. Canada	5.4	4.6	3.7	1.6	9.8	1.5	5.3	4.3	3.2	6.2	19.9	2.7	6.3	3.8	0.5	1.4	1.3	0.5	3	85
Middle Atlantic	2.6	5.1	2.4	2.2	12.6	7.7	15.3	14.0	4.1	12.6	21.0	4.4	4.7	3.3	2.7	5.4	3.4	2.7	18	144
N.E. Central	4.0	3.6	2.6	3.4	10.0	2.0	10.0	11.1	1.9	9.4	20.7	3.2	4.5	3.8	0.5	4.5	2.3	1.9	11	111
N.W. Central	1.9	1.9	0.3	1.0	6.8	1.7	3.9	6.5	1.7	5.5	4.9	2.6	5.3	1.5	0.2	1.0	2.2	1.5	4	55
South Atlantic	2.0	3.2	1.6	0.7	3.6	1.0	2.7	3.4	1.7	6.0	9.3	1.0	1.0	0.2	—	2.0	0.2	0.4	3	42
S.E. Central	0.3	0.3	—	0.5	1.8	0.8	1.0	0.5	0.5	0.7	1.0	—	—	—	0.5	—	—	—	4	12
S.W. Central	0.8	—	—	—	0.8	0.3	2.0	0.8	—	0.5	4.3	0.5	—	—	—	—	—	0.8	—	12
Mountain	1.7	0.3	1.0	0.2	2.4	—	1.0	2.6	0.2	0.6	2.6	—	—	—	0.2	1.0	—	—	—	14
Pacific, Brit. Col. and Hawaii	2.3	1.8	1.3	1.4	3.5	1.3	3.9	3.1	1.2	3.8	3.8	3.7	1.6	—	0.2	1.4	0.2	1.2	1	40
Total	21.0	20.9	13.0	10.9	51.4	16.6	45.2	46.4	14.4	45.2	87.5	18.2	23.5	12.5	4.9	16.7	9.8	9.1	44	515

TABLE V

FIELDS OF RESEARCH (WOMEN)

	Psychology																			
	General	Abnormal	Psychopath.	Applied	Tests	Industrial	Clinical and Vocational	Educational	Animal	Social and Religious	Experimental	Genetic and Child	Theoretical	Philosophy	Aesthetics	Education	Physiology and Neurology	Statistics	No Research	Total
New England and E. Canada	1.2	—	0.3	0.3	2.1	0.7	2.0	1.2	—	0.4	4.4	1.0	0.3	—	—	2.0	—	—	6	21
Middle Atlantic	—	1.5	1.5	—	5.5	1.6	16.8	7.3	0.9	2.0	2.4	2.4	0.5	0.5	1.2	2.4	—	0.3	3	50
N.E. Central	1.7	1.4	1.4	—	7.4	0.8	9.8	3.7	—	1.2	4.9	3.2	—	—	0.8	0.3	—	0.3	9	46
N.W. Central	0.5	—	—	0.3	2.2	—	1.7	1.2	—	0.7	0.3	1.8	0.3	—	—	0.2	—	0.5	2	12
South Atlantic	—	0.5	2.2	—	2.0	—	1.5	—	1.0	—	5.0	1.0	—	—	—	—	0.8	—	1	15
S.E. Central	—	—	—	—	0.3	—	—	0.3	—	0.3	1.0	—	—	—	—	—	—	—	—	2
S.W. Central	—	—	—	—	—	—	—	0.3	—	—	0.8	0.3	—	—	—	—	—	0.5	—	2
Mountain	—	—	—	—	0.8	—	—	1.0	—	—	0.8	—	—	—	0.3	—	—	—	—	3
Pacific, Brit. Col. and Hawaii	—	1.7	0.5	—	3-1	0.2	6.1	2.7	—	0.6	0.5	2.8	—	—	0.5	—	—	0.2	3	22
Total	3.4	5.1	5.8	0.7	23.5	3.4	37.9	17.8	1.9	5.1	20.2	12.7	1.2	0.5	2.9	4.9	0.8	1.9	24	173

TABLE VI

FIELDS OF RESEARCH (MEN AND WOMEN)

	Psychology													Philosophy	Aesthetics	Education	Physiology and Neurology	Statistics	No Research	Total
	General	Abnormal	Psychopath.	Applied	Tests	Industrial	Clinical and Vocational	Educational	Animal	Social and Religious	Experimental	Genetic and Child	Theoretical							
New England and E. Canada	6.6	4.6	4.0	1.9	11.8	2.2	7.3	5.4	3.2	6.6	24.3	3.7	6.6	3.8	0.5	3.4	1.3	0.5	9	106
Middle Atlantic	2.6	6.6	3.9	2.2	18.0	9.2	32.2	21.4	5.0	14.5	23.4	7.0	5.2	3.8	3.9	7.7	3.4	3.0	21	194
N.E. Central	5.6	5.0	4.0	3.4	17.4	2.8	19.8	14.9	1.9	10.5	25.6	6.5	4.5	3.8	1.4	4.8	2.3	2.2	20	157
N.W. Central	2.4	1.9	0.3	1.3	9.0	1.9	5.6	7.8	1.7	6.2	5.2	4.4	5.7	1.5	0.2	1.2	2.2	2.0	6	67
South Atlantic	2.0	3.7	3.7	0.7	5.6	1.0	4.2	3.4	2.7	6.0	14.3	2.0	1.0	0.2	—	2.0	1.0	0.4	4	57
S.E. Central	0.3	0.3	—	0.5	2.2	0.8	1.0	0.8	0.5	1.0	2.0	—	—	—	0.5	—	—	—	4	14
S.W. Central	0.8	—	—	—	0.8	0.3	2.0	1.2	—	0.5	5.2	0.8	—	—	—	—	—	1.3	—	14
Mountain	1.7	0.3	1.0	0.2	3.4	—	1.0	3.6	0.2	0.6	3.5	—	—	—	0.5	1.0	—	—	—	17
Pacific, Brit. Col. and Hawaii	2.3	3.5	1.8	1.4	6.6	1.6	10.0	5.8	1.2	4.4	4.3	6.6	1.6	—	0.8	1.4	0.2	1.4	4	62
Total	23.2	25.9	18.8	11.6	74.9	19.9	83.1	64.2	16.3	50.3	107.6	30.9	24.6	13.0	7.8	21.6	10.6	11.0	68	688

Research Interests

Perhaps the research interests of the psychologists may give a truer picture of trends in American psychology than the subjects of instruction because so many members of the Association are now not giving instruction. An analysis of the fields of research are given in Tables IV.–VI., for men, women, and men and women combined respectively. The geographical sections are the same as those described above.

For the purpose of this analysis, we have made a much greater delimitation of fields than we employed for the consideration of subjects of instruction. So far as possible we have kept our definitions similar to those used by Boring. *General Psychology* includes unqualified reports of psychology as a field of research and a few special cases such as analytical and synthetic psychology of too infrequent occurrence to be listed separately. *Applied Psychology* includes all application not listed elsewhere (*e.g.*, advertising, etc.). *Tests* include mental tests, educational tests, industrial tests and a few mentions of individual psychology. *Industrial Psychology* includes personnel but not industrial tests. *Clinical and Vocational Psychology* have been listed together but do not include intelligence tests. *Animal Psychology* includes animal behavior and comparative psychology—the latter term seemingly used in the Year-Book only for infra-human material. *Social and Religious Psychology* was a classification not employed by Boring who included the (then) few cases under *General*. *Experimental Psychology* is usually reported as such but such special experimental interests as optics and emotion are included under it. *Theoretical Psychology* includes systematic psychology and the history of psychology. A new class of *Child and Genetic Psychology* has been added in which the genetic aspect refers to human material only. *Philosophy* includes logic, ethics and metaphysics. *Education* includes experimental education and hygiene but not educational tests. *No Research* means that there was no report of any research interest.

When one person reported more than one field of research interest, the different fields were counted equally. Thus a

report of two fields scored each 0.5; three fields each 0.33 and so on. Quite a number of persons reported five fields of research and one individual reported as many as eight! The section of *Anthropology* employed by Boring has been eliminated.

TABLE VII

FIELDS OF RESEARCH (PER CENT)

	Men		Women		Men and Women	
	1920	1928	1920	1928	1920	1928
Experimental	24	20	26	13	25	18
Applied	35	43	44	62	37	48
Theoretical	15	17	5	6	13	14
Not Psychology	10	10	9	6	10	9
No Research	13	9	15	14	14	10

These tables are rather too large and cumbersome to be the basis for discussion and hence a supplementary Table VII. has been constructed. We have combined the various fields of research used in Tables IV.–VI. under five categories. *Experimental* includes animal behavior and all kinds of experimental psychology. *Applied* includes abnormal, psychopathology, applied, tests, industrial, clinical and vocational, educational, and genetic and child psychology. Probably some of the abnormal and child psychology might properly be included under experimental but the effect on the general averages would be negligible. *Theoretical* includes general, theoretical, social and religious psychology. *Not Psychology* includes philosophy, æsthetics, statistics, education, psychiatry, physiology and neurology. The numbers in the tables indicate percentages of individuals of the total membership interested in these different fields of research. The figures for 1920 are compared with those for 1928 in adjoining columns for men and women alone and for men and women combined.

A study of this table indicates that the percentage of individuals interested in *theoretical* psychology or in some related field (*not psychology*) has remained very constant over

the eight year period. There has been a slight decrease in the percentage of individuals not reporting research.

As was the case with subjects of instruction considered above, it is with *experimental* and *applied* psychology that definite trends are to be noted. In the first place there has been a significant decrease in the percentage of individuals interested in experimental psychology. This is especially evident among the women for whom the percentage has been just cut in half (from 26% in 1920 to 13% in 1928). Also there has been a very marked increase of interest in research in the different fields of applied psychology. Again this is more evident among the women than the men. Indeed both in 1920 and in 1928 there was a much greater tendency for women than for men to go into applied psychology. The men tended somewhat more toward experimental psychology. This tendency has been increased during the last eight years. The reason for this seems fairly obvious. Experimental psychology is much more nearly coördinated with academic positions than much of the applied psychology and relatively few academic positions are as yet open to women.

It seems worth while to indicate the number of individuals who reported, either in whole or part, the different field of research for the 620 who reported any research interest. In rank order, these are given in the following table:

Field of Research	Total Number Interested	Only Interest Reported
Tests	205	7
Experimental Psychology	200	46
Clinical and Vocational	195	17
Educational Psychology	169	11
Social and Religious	90	16
Abnormal Psychology	78	3
Genetic and Child	74	7
Theoretical Psychology	59	3
General Psychology	54	7
Education	54	7
Industrial Psychology	47	5
Psychopathology	47	5
Statistics	37	—
Animal Psychology	36	5
Applied Psychology	30	—
Philosophy	23	7
Aesthetics	21	—
Physiology and Neurology	21	5

A consideration of this table indicates that a greater number of individuals are interested in tests than in any other field of research but very few such individuals seem to be interested in tests solely for their own sake. On the other hand, almost 25% of the individuals reporting experimental psychology indicate that it is their only field of research interest. One more interesting trend is to be noted, namely the increased interest in religious and social psychology. All of the 16 individuals who indicate this as their sole field of research interest are men.

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THE METHOD OF ABSOLUTE JUDGMENT IN PSYCHOPHYSICS

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Introduction

In certain fields of psychological investigation, chiefly those of feeling and esthetics, there has sprung up, in response to the need for quantification of a special sort, a method of measurement which should rightly be included among the recognized psychophysical methods. The method in its modern form has no generally accepted name; we propose here to call it *the method of absolute judgment.* The name is suggested by the distinguishing characteristic of the method, that of asking for the rendering of absolute judgments upon single supraliminal stimuli; other methods applicable to supraliminal stimuli require judgments of relation between two (or more) stimuli presented together.

The method of absolute judgment ordinarily is characterized further by its psychophysical function, which is that of determining the rank-order of the members of a group of stimuli; as, *e.g.* the determination of the relative order of pleasantness of the several members of a group of odors. Whenever, on the other hand, a measure of differential sensitivity is required, one of the relative methods is used.[1]

Fundamentally speaking, however, a measure of differential sensitivity is obtainable through any procedure that yields a frequency-distribution of judgments of a certain class as a function of a stimulus-scale. Such a distribution is not necessarily a function of the comparison of stimuli with a fixed standard. It is perfectly possible, as will appear presently, to obtain such a distribution of judgments, and

[1] The distinction is not without exception: *the method of paired comparisons* is a relative method, yet, like the absolute method, its purpose is a determination of the rank-order of stimuli as regards some common feature.

hence a measure of differential sensitivity, through the absolute judgment of single stimuli. Thus it must follow that from considerations of theory the distinction between relative and absolute methods in terms of psychophysical function is not an essential one.

Notwithstanding, the absolute method has been singularly neglected in most discussions of psychophysical procedure. It has had but limited use, and no clear theoretical formulation. Yet the method seems to possess practical possibilities. We propose, accordingly, to enter into a discussion of the method, first, from the viewpoint of its theoretical foundations; and, second, from the viewpoint of the results which it yields as compared with those of another psychophysical method in a field in which each is applicable.

History of the Method of Absolute Judgment.—Of what we may call the general method of absolute judgment there have appeared three principal forms. The first of these, *the method of choice*, was introduced by Fechner in 1871 as a method for experimental investigation in esthetics.[2] According to the method, each of a number of *O*s was required to choose the most pleasing of a group of simultaneously presented stimuli. The arrangement of the stimuli before each *O* was haphazard, and was varied for different *O*s; the relative agreeableness of each stimulus of the group was expressed in terms of the number of first choices which it received.[3]

The serial method appeared in Witmer's study of esthetic forms in 1894 as a modification of the method of choice.[4] The stimuli, instead of being placed before the *O* in haphazard order, were arranged in a definite, 'natural' series; color

[2] Fechner, G. T., 'Zur experimentalen Aesthetik,' *Abh. d. k. sächs. Gesellsch. d. Wiss., math.-phys. Cl.*, 1871, 9, 602*f*.

[3] For illustration of the technique see Fechner, 'Vorschule der Aesthetik,' 1879, I, 193*f*.

[4] Witmer, L., 'Zur experimentellen Aesthetik einfacher räumlicher Formverhältnisse,' *Phil. Stud.*, 1894, 9, 122*ff*., 209*ff*. In his treatment of affective methods Külpe a little earlier had discussed what he called a 'serial method,' and had wrongly identified it with Fechner's method of choice ('Grundriss der Psychologie,' 1893, 239*f*., 244). Külpe's method really involved a new feature, the requirement of the *O* to arrange the stimuli in order of preference. (In later discussions this has been called *the method of arrangement*.)

stimuli, for example, were arranged in order of brightness, or in spectral order, while forms were placed in order of size, or area, or the like. The *O* then was required to indicate not only his first preference among the stimuli, but his second, third, fourth, and so on, throughout the entire series. This procedure gave an order of preference for each *O*, and hence diverged from the method of choice, which gave but a composite order for a group of *O*s.

A year later, in a study of the affective tone of colors, Major used the serial method, but with two important modifications. Arguing that the simultaneous presentation of a series of colors produces undesirable contrast-effects, he resorted to the presentation of the colors one at a time, but in a temporal order corresponding to their position in the spectrum.[5] And, since in a series spread out in time the *O*s obviously were unable to indicate their order of preference in any direct manner, Major had them judge each stimulus as it appeared in terms of an arbitrary scale running from '1,' very pleasant, through indifference to '7,' very unpleasant. By adding up the scores which each stimulus received in the course of a given number of presentations of the series, and then ranking the stimuli in order of their total scores, the relative affective value of each stimulus was determined. It is plain that the changes instituted by Major considerably altered the task set for the *O*; indeed, Major himself was dubious whether his procedure could still be regarded as a serial method.

Cohn, in repeating Major's work, further modified the method by the introduction of a certain degree of haphazardness in the presentation of the stimuli.[6] With the use of utterly haphazard presentation, which has come later, the method is so much altered that no longer can it be termed

[5] Or in order of saturation, etc. Major, D. R., 'On the affective tone of simple sense-impressions,' *Amer. J. Psychol.*, 1895–6, **7**, 57*ff*.

[6] He retained a serial order of presentation of the brightnesses of a given hue, but the presentation as regards hue was haphazard. Cohn, J., 'Gefühlston und Sättigung der Farben,' *Phil. Stud.*, 1900, **15**, 279–286.

serial; practically it is a new method, and one which may properly be called *the method of absolute judgment.*[7]

A consideration of the development of the absolute method suggests that the character of the judgment which it requires is at all times fundamentally the same. The judgment expresses a relationship between a particular stimulus and a large series of stimuli presented with or prior to it. In the method of choice the assumption that the series provides the basis of judgment is implicit. With Fechner, as also with Witmer, all the members of the series were simultaneously presented. But with Major they were spread out in time; and here a problem appeared, the problem of the establishment and preservation of the series for the *O*. The problem was solved in Major's case by a maintenance of the stimuli in orderly sequence, with *O*'s knowledge, obviously to facilitate his conception of them as a series.[8] But when Cohn mixed up the stimuli he—unwittingly, no doubt—reintroduced the problem of the establishment and functioning of the series for judgment. The problem is still with us: how can the haphazard presentation of single stimuli bring about such a conception of the series as a whole as will serve as an adequate basis of judgment?

The Absolute Factor in the Relative Judgment.—Essentially the same question as the one just formulated emerges from a careful analysis of the relative judgment. According to older theory, the comparative judgment arises from the relating of two sense-magnitudes simultaneously present to mind. With simultaneous presentation of the stimuli the process of comparing them seemed to present no difficulty. But when presentation was successive there arose the problem of the persistence of the first impression for the comparison.

With Fechner originated the theory that the first impression is preserved in the form of a memory-image on which the second is superimposed.[9] For almost forty years

[7] The use of completely haphazard presentation was first suggested by Cohn, see *op. cit.*, p. 286. For a recent use of the method in its modern form, see Young, P. T. 'Constancy of affective judgments to odors,' J. EXPER. PSYCHOL., 1923, 6, 182*f*.

[8] Major is careful about the point; see *op. cit.*, 59.

[9] 'Elemente der Psychophysik,' 1860, I, 88.

the Fechnerian theory held sway,[10] until in 1898 Schumann subjected it to attack, maintaining on the basis of introspective evidence that in the majority of cases the presence of a memory-image in the process of comparison cannot be demonstrated.[11] He did not deny some after-effect of the first impression; but he insisted that it need not be conscious.

Schumann's contention was supported in Martin and Müller's experiments with lifted weights, for there it was found that reliable judgments upon a stimulus may be made without obvious reference to a preceding standard stimulus.[12] The basis of judgment in such cases was said to be an absolute impression; *i.e.* a lifted weight gave rise to an absolute impression, say, of heaviness. That such absolute impressions played an important part in their psychophysical situation was indicated by characteristic tendencies in the judgments, tendencies which could not be explained in terms of the familiar Fechnerian errors.[13] The findings in lifted weights were verified by later investigators, and the influence of the absolute impression was demonstrated in a number of other fields.[14]

The psychological basis of the absolute impression remains to be defined. In the discussion of Martin and Müller the suggestion appears that it is a function of the physical characteristics of the *O* and of his past experience in dealing with objects. That experience with stimuli in daily life is a factor in determining the absolute impression seems a reasonable claim, but it is likely also that during an experi-

[10] Cf., *e.g.* Stumpf, C., 'Tonpsychologie,' 1883, I, pp. 98*f.*; Lehmann, A., 'Kritische und experimentelle Studien über das Wiedererkennen,' *Phil. Stud.*, 1892, 7, 205.

[11] Schumann, F., 'Zur Psychologie der Zeitanschauung,' *Zsch. f. Psychol.*, 1898, 17, 118*ff.*; Beiträge zur Analyse der Gesichtswahrnehmungen III, Der Successivvergleich, *ibid.*, 1902, 30, 241–291. See also, Külpe, O., 'Aussichten der experimentellen Psychologie,' *Phil. Monatshefte*, 1894, 30, 282*f.*; Angell, F., 'Discrimination of clangs for different intervals of time,' *Amer. J. Psychol.*, 1900, 12, 69*ff.*

[12] Martin, L. J., and Müller, G. E., 'Zur Analyse der Unterschiedsempfindlichkeit,' 1899, 43*ff.*

[13] The tendencies were of two kinds, a general tendency which was the same for all *O*s, and typic tendencies which varied for different *O*s. See *op. cit.*, 21*ff.*

[14] In the following, at least: lights, esthesiometry, time-intervals, clangs, sound-intensities, and visual distances. Titchener gives the references: 'Experimental Psychology,' 1905, II, ii, p. 304, note 5.

ment the members of the series presented would have considerable influence. It seems probable that the absolute impression is no constant affair, but is responsive to a change in the magnitude or the range of the stimuli used in a given situation. Indeed, we are confronted here with the same problem that arose in our consideration of the absolute judgment, the problem of the manner of establishment, through the presentation of members of a stimulus-series, of a basis for the absolute factor in judgment.

The Absolute Series

If, as we have stated, an absolute judgment expresses a relationship between a present stimulus and a whole series of stimuli, then it is plain that the ability of the *O* to make the judgment presupposes some sort of knowledge of that series. His previous experience with a body of stimuli of ranging magnitudes makes possible the assignment to an incoming stimulus of a more or less definite position in the total range of magnitudes. Just what is the nature of the mental formation through which knowledge of the series is carried is a problem of considerable interest. Introspection discloses that at times the series is represented by a mental image, and that in rendering a judgment the *O* relates the incoming stimulus to this image. Frequently, however, the presence for the *O* of any imaginal representation of the series fails to be revealed, though absolute judgments are made with complete readiness. Indeed, as an experiment progresses and an *O* becomes automatized in his task, the general tendency seems to be for any imaginal representation to disappear. There is in such case, however, no impairment of *O*'s capacity to judge; there is no noticeable change either in the distribution of the judgments or in the ease of making them. Clearly, introspection is no safe index of the presence of a mental formation adequate to the absolute judgment: the judgment appears in the complete absence of any content obvious to introspection. One must infer the presence of such a mental formation simply on the basis of the *O*'s capacity to judge.

The mental formation that serves as a basis for the rendering of absolute judgments we propose to call an *absolute series*. The expression reflects the essential characteristic of the formation: it suggests a correspondence between the actual range of stimuli and *O*'s conception of the stimuli as a series. Such a correspondence is a fact that is readily demonstrated in an experimental situation. After the presentation of a series of stimuli, say, of squares ranging from 10 to 20 cm. on a side, the *O* retains a conception of the stimuli as a series; and this is true whether in the presentation they have appeared in order of magnitude or have been quite haphazard. *O* will be able to demonstrate his knowledge of the general order of magnitude of the stimuli, of the approximate range of magnitudes, and perhaps also something of the internal structure of the series. In the above case, for example, he will be able roughly to indicate, by drawing or description, the general size of the squares, their variation in size, and possibly that, let us say, the smaller squares appeared with the greater frequency.

The absolute series should be considered in relation to the formation, not only of absolute judgments, but of relative judgments also. As shown above,[15] work with the relative method yields evidence suggesting that in certain instances an absolute series becomes established and plays an important rôle in determining judgment. A case in point is Martin and Müller's absolute impression, which can readily be accounted for in terms of our theory as a relation between the given stimulus and a previously established absolute series.

The Establishment of an Absolute Series.—In the use of the absolute method in psychophysics one finds that the *O* can make adequate judgments upon the members of a stimulus-series only after he has become familiar with the general range of that series. In certain settings, as, *e.g.* in experiments on affection where judgments of pleasantness or unpleasantness are required, familiarity with the stimulus-range has already been built up in past experience, and

[15] p. 470.

in judging the *O* is, in effect, relating the present stimuli to that range.[16] Usually, however, the series of stimuli concerned in the experiment at hand must be presented entire or in large part before the *O*'s judgment can be considered reliable.

The writers have in practice found it to be good technique to require the *O* to begin judging with the very first stimulus presented, notwithstanding the fact that at this time he may have no notion of the stimulus-range. If in the attempt to establish an absolute series the stimuli are presented without any requirement of judging, considerable time is wasted. The attitude of experiencing without judging is different from that of experiencing to judge. The second attitude favors the early growth of *O*'s confidence in his adequacy to the task; while the first attitude, on the other hand, leads him (particularly if he be unpracticed) to demand an unduly secure grasp of the series before committing himself in judgment. It is clear in theory that for preliminary presentation to be of greatest effectiveness it must occur under precisely the same circumstances and under the same attitude of the *O* as are intended for the experiment proper, and that any clue to the *O* of a transition from preliminary to main experiment should be strictly avoided. Actually, the point of transition can best be determined by the *E*, after the experiment is concluded, on the basis of a statistical examination of the data.

The requirement of judging before an absolute series has become established presents no practical difficulty. The first judgment, it is true, must be merely random. Ordinarily the *O* responds with the middle of possible categories. After a strikingly small number of presentations, however, his judgments become stabilized, showing the ready development of the absolute series. The rapid growth of the absolute series is favored by the early presentation of the extreme members of the body of stimuli. At no time, strictly speaking, can it be regarded as established finally; the fact is obvious that

[16] Strictly, this statement holds only at the beginning of an investigation, for as experimentation proceeds the judgments become more and more a function of the particular stimuli presented.

it is a continuous function of stimulus-presentation. Practically, however, with the same set of stimuli the change after the first preliminary period is inappreciable.

The rate of establishment of the absolute series no doubt varies with the kind of stimuli used and with the arrangement of the stimulus-series. In the field of tonal pitch the rate of development of a basis for absolute judgment has been investigated by Truman and Wever,[17] who presented singly five different tones by the method of constant stimuli, and showed that reliable absolute judgments occurred after but very few presentations. Indeed, the absolute series was so readily established that, without any preliminary practice, results from the first 25 presentations were indistinguishable from those of the second 25. The present writers have observed a similar phenomenon in the field of lifted weights; [18] here also reliable absolute judgments occur after but a few presentations. A full and systematic investigation of the factors influencing the rate of establishment of an absolute series has yet to be made.

Persistence of the Absolute Series.—The extent to which an absolute series may hold over from one experimental session to the next is a second problem, and, here particularly, empirical evidence is meager. Moreover, a precise determination of the amount of persistence of the absolute series in any given case will be difficult in proportion to the rapidity of its initial establishment. The measure of persistence must be expressed in terms of a difference between the rates of establishment of the absolute series in successive sittings, and the greater the initial rate the less the possible difference and hence the greater the likelihood of an obscuring of a difference by experimental error. In the use of the absolute method with weights and with tonal sequences judged as to finality the writers have observed that reliable judgments appear earlier in successive sessions of an investigation, suggesting that the absolute series persists in some degree from one session to the next. These observations are but

[17] Truman, S. R., and Wever, E. G., 'The judgment of pitch as a function of the series,' *Univ. Calif. Publ. Psychol.*, 1928, 3, 215–223.

[18] *Infra*, p. 477.

casual, however; the problem calls for direct experimental attack in a variety of fields.

Indirect evidence of the persistence of an absolute basis of judgment may be obtained from a consideration of the results of the relative method when the time-interval between standard and comparison stimuli is large. It has been shown in a number of experiments [19] that delay as long as one to five minutes in the presentation of the second stimulus does not greatly reduce the accuracy of judgment. The same is true even when the interval is filled with distraction. The indications are that judgments under these conditions are not relative in the usual sense, but are absolute. Indeed, it was reported in the study by Fullerton and Cattell [20] that the standard did not persist through the interval in the form of a memory-image, but rather that each stimulus was 'decided' separately and that these 'decisions' were compared for judgment. Angell [21] had a similar explanation; the judgments were designated as 'free' in the sense of lacking any imaginal basis (*cf.* Martin and Müller's 'absolute impression'). In the writers' view these facts are to be explained in terms of the establishment, and preservation through the interval, of an absolute series which serves as a basis for judgment.

Responsiveness to a Change in the Stimulus-Series.—Though it seems probable that the absolute series may remain relatively stable over a considerable interval, it may show nevertheless a ready response to the presentation of a new range of stimuli. This fact is illustrated by the following experiment.

Using an absolute procedure (outlined in Experiment II. below [22]), an *O* was given a 'light' series of weights (84, 88, 92, 96, and 100 grams) and after his judgments had become stabilized, a 'heavy' series (92, 96, 100, 104, and 108 grams)

[19] Fullerton, G. S., and Cattell, J. McK., 'On the perception of small differences,' 1892, pp. 147*ff.*; Baldwin, J. M., and Shaw, W. J., 'Memory for square size,' PSYCHOL. REV., 1895, 2, 236–9; Angell, F., and Harwood, H., 'Experiments on discrimination of clangs for different intervals of time,' *Amer. J. Psychol.*, 1899, 11, 67–79; Angell, F., *op. cit.* in note 11, 58–79.

[20] *Op. cit.*, p. 149.

[21] *Op. cit.*, p. 69*f.*

[22] p. 481.

suddenly was introduced. The effect of the first series on the judgments of the second was quite evident for 20 or 25 presentations, *i.e.* for four or five rounds judgments of 'heavy' predominated for all the stimuli; from this point on, however, the judgments showed a redistribution conforming to the second stimulus-series.

Results similar to the above were shown in the experiment of Truman and Wever,[23] in which a 'normal' series of five pitches was followed by a 'displaced' series, and the judgments after two or three rounds showed a redistribution corresponding to the new set of stimuli.

The effects of shifting the stimulus-series have been disclosed in other fields. In the estimation of magnitudes, Hollingworth has found that the constant errors of estimation are a function of the series of stimuli.[24] The greater magnitudes tend to be underestimated and the smaller magnitudes overestimated, with an indifference-point in the middle of the series. If the stimulus-series is shifted or extended the distribution of errors shifts accordingly, which means that the indifference-point maintains a constant relation to the series as a whole. The phenomenon is clearly a function of the presence of a series of stimuli, for considerable constant errors do not occur in the estimation of a magnitude taken out of relation to a series. These findings Hollingworth explains as due to a 'central tendency of judgment,' a tendency for the judgment upon each magnitude in the series to be deflected toward the midpoint of that series.[25] According to Hollingworth, the deflection of judgments is produced through the operation of a mental set corresponding to the stimulus-series. This 'mental set' is essentially what we have called an absolute series, and in Hollingworth's data its ready responsiveness to an extension or shift of the stimuli is clearly evident.

[23] *Op. cit.*

[24] Hollingworth, H. L., 'The inaccuracy of movement,' *Arch. Psychol.*, 1909, 2, No. 13, 21–39; 'The central tendency of judgment,' *J. Phil.*, 1910, 7, 461–9.

[25] A similar phenomenon had been noted earlier by Warren and Shaw in the comparison of squares, 'Further experiments on memory for square size,' PSYCHOL. REV., 1895, 2, 239–244.

In the above discussion the absolute series has been postulated as a basis for the formation of absolute judgments, and certain of its fundamental properties have been considered. The significance of the absolute series in experimental situations will be revealed in the application of the method of absolute judgment to the solution of psychophysical problems.

Psychophysical Characteristics of the Method of Absolute Judgment

The method of absolute judgment has developed in response to the need for quantification in fields in which other measurement-methods are ineffective.[26] The use of the method has always been rather restricted, which may in part explain the fact that there has been no thoroughgoing comparison of its results with those yielded by other psychophysical methods.[27] It is quite apparent that such a comparison would lead to a clearer evaluation of the method and of its systematic position in the psychophysical field. Consequently, in experiments with lifted weights the writers have sought to compare results obtained by the method of absolute judgment with results obtained by the method of constant stimuli. Two experiments were performed, differing slightly in their settings.[28]

Experiment I

As *stimuli* were used a number of weights: 88, 92, 96, 100, 104 grams; the weights were of hard rubber, $2\frac{1}{2}$ in. in diameter and 1 in. high, and were arranged on a weight-table in the usual manner. The order of presentation was regulated so as to neutralize the effect of contrast.[29]

[26] See, however, Note 1.

[27] There has been some comparison of the method of paired comparisons with forms of the absolute method. See Witmer, L., *Phil. Stud.*, 1894, **9**, 122*ff.*, and Cohn, J., *Phil. Stud.*, 1900, **15**, 282*ff.*

[28] The first experiment was performed in the Harvard Psychological Laboratory in 1926, the second in the Princeton Psychological Laboratory in 1927.

[29] For a description of the weights, see Fernberger, S. W., 'A new form of stimuli for lifted weight experiments,' *Amer. J. Psychol.*, 1920, **31**, 147–151. For the regulation of contrast-effects, see Fernberger, 'Interdependence of judgments within the series for the method of constant stimuli,' J. Exper. Psychol., 1920, **3**, pp. 127, 148–9.

The *O*s were H. R. De Silva (*D*), H. E. Israel (*I*), R. A. McFarland (*M*), F. A. Pattie (*P*), and the writers (*W* and *Z*).

There were two *procedures*. In the procedure with the method of constant stimuli the 100-gram weight was given as standard, and was always lifted first. The *O* was instructed to judge, with the criterion of objective weight,[30] in terms of 'heavier,' 'equal,' and 'lighter'; in case of a judgment of 'doubtful' the presentation was repeated. In the procedure with the method of absolute judgment the five weights were used without any standard. The *O* was required to make absolute judgments, using in his report the categories 'one,' 'two,' and 'three' to include the series from light to heavy in the order named, with the step from 'one' to 'two' equal to that from 'two' to 'three.'

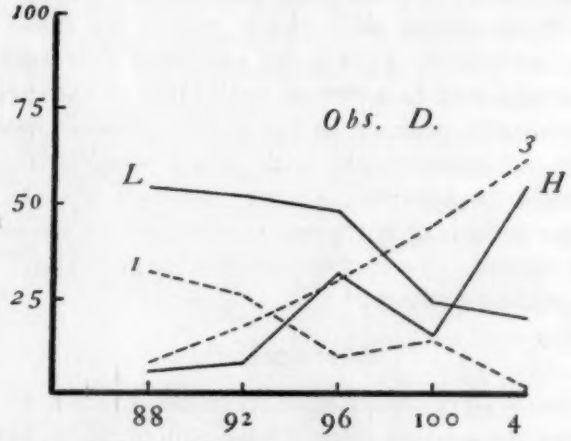

FIG. 1. Relative curves in solid lines, absolute curves in broken lines.
See text, p. 479.

Every *O* was used with both of the above procedures; for *I*, *P*, and *Z* the experimental session with absolute judgments preceded that with relative; for the remaining *O*s the order was reversed. Except for *W* and *Z* the *O*s were without knowledge of the object of the investigation. In both procedures the *O* was given a short period of preliminary practice,

[30] Fernberger, S. W., 'An experimental study of the "stimulus error,"' J. EXPER. PSYCHOL., 1921, 4, 63–76.

and then 50 judgments were taken on each stimulus (or comparison-pair).

Results. For every *O* the distributions of judgments with both relative and absolute methods are shown in the form of curves, Figs. 1–6. Stimulus-values are shown in grams

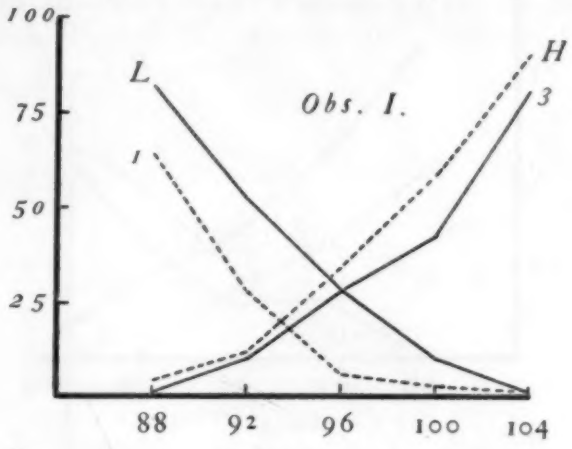

FIG. 2.

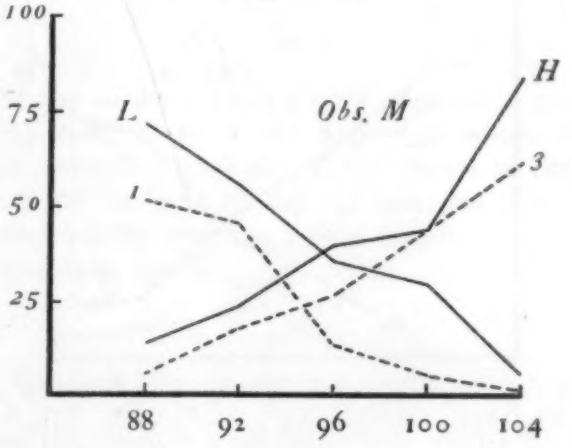

FIG. 3.

along the abscissa, and frequencies of judgment in per cent along the ordinate. The curves for the relative method are drawn in solid lines, those for the absolute method in broken lines. The symbols indicate the judgment which the par-

ticular curve represents: 'lighter' (*L*), 'heavier' (*H*), 'one' (*1*), 'three' (*3*).

The data obtained by the relative method were treated statistically by the customary constant-stimulus process (after

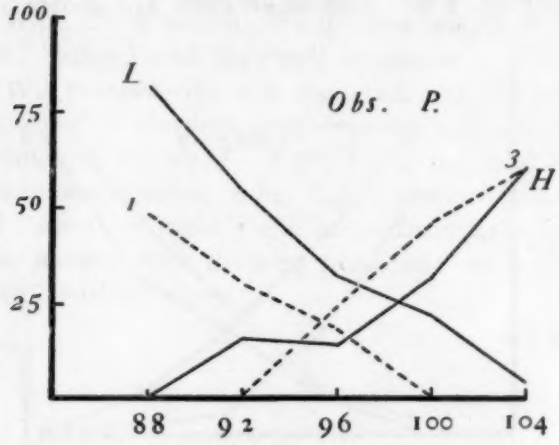

FIG. 4.

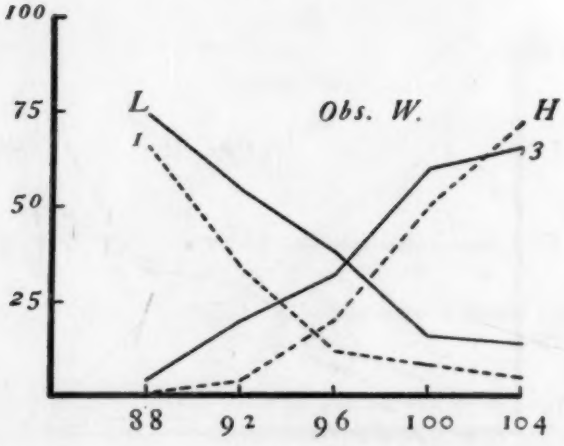

FIG. 5.

Urban), the values of *h*, the measure of precision, and of *C*, the 50 per cent crossing-point of the ogive fitted to the distribution, being determined for judgments of 'lighter' and of 'heavier' for every *O*. Moreover, for purposes of comparison, like values were computed from the distributions of

judgments of 'one' and 'three' as obtained by the absolute method.

The results of the above calculations are shown in Table I. In the first column is given the name of the *O*. Column 2 indicates whether the method of procedure was relative (Rel.) or absolute (Abs.). Column 3 gives the categories of judgment: 'lighter' (*L*), 'heavier' (*H*), 'one' (*1*), 'three' (*3*).

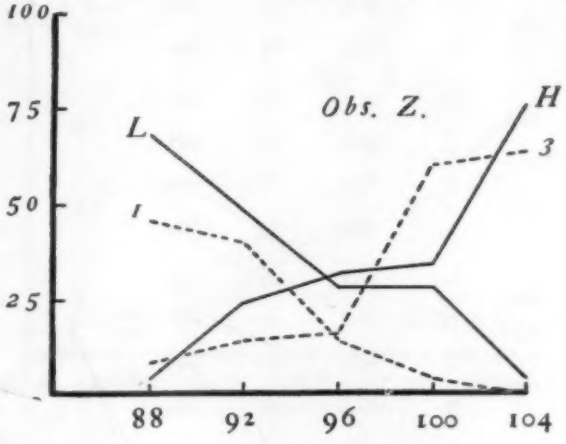

FIG. 6.

C and *h* occur in the 4th and 5th columns, respectively. The remaining columns show the degree to which the curves approach normality (column 6), the values of the intervals of uncertainty (column 7), and the measures of the points of subjective equality (column 8); the significance of these will be made clear in the discussion below.

EXPERIMENT II

As stimuli in this experiment seven weights were used: 84, 88, 92, 96, 100, 104, and 108 grams. The *O*s were C. W. Bray (*B*) and H. Schlosberg (*S*).

The instructions for the relative procedure were as in Experiment I. For the absolute procedure they differed from the previous ones only in that the categories of judgment were 'heavy,' 'medium,' and 'light'; and in that no instruction was given as to the steps between the categories.

Table I

(For explanation, see p. 481)

			C	h	$\Sigma(p' - p)^2$	IU	PSE
D	Rel.	L	91.6	.05	.022	9.3	99.3
		H	104.6	.07	.047		
	Abs.	1	82.3	.05	.007	18.9	93.2
		3	101.2	.07	.000		
I	Rel.	L	92.5	.12	.002	7.4	96.1
		H	99.9	.12	.011		
	Abs.	1	89.5	.15	.001	8.7	93.5
		3	98.2	.13	.004		
M	Rel.	L	93.4	.08	.009	4.7	95.8
		H	98.1	.08	.038		
	Abs.	1	89.4	.10	.015	12.1	94.8
		3	101.5	.08	.004		
P	Rel.	L	93.4	.13	.016	9.6	97.1
		H	103.0	.08	.017		
	Abs.	1	87.4	.08	.009	14.1	94.8
		3	101.5	.08	.021		
W	Rel.	L	93.1	.08	.005	5.4	95.8
		H	98.5	.08	.008		
	Abs.	1	90.0	.11	.005	10.6	95.5
		3	100.5	.13	.002		
Z	Rel.	L	92.0	.08	.014	8.2	96.2
		H	100.2	.08	.017		
	Abs.	1	87.0	.07	.015	13.4	94.5
		3	100.5	.09	.033		

Each *O* served under both procedures; the procedures were alternated in separate experimental sessions until 100 rounds of one judgment per stimulus (or comparison-pair) were obtained under each.

In all respects besides those indicated the conditions in Experiment II. were the same as in Experiment I.

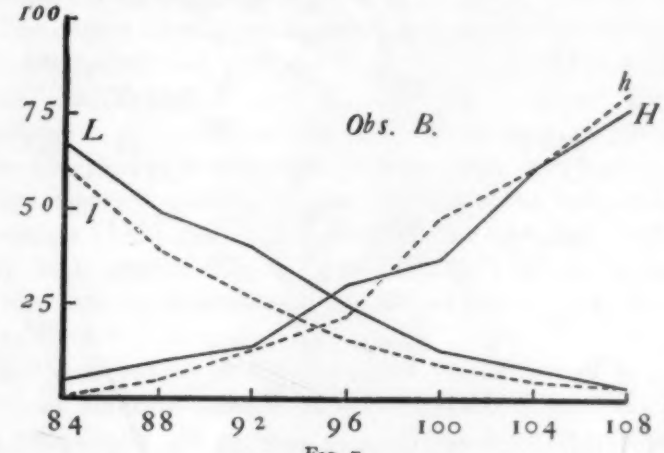

FIG. 7.

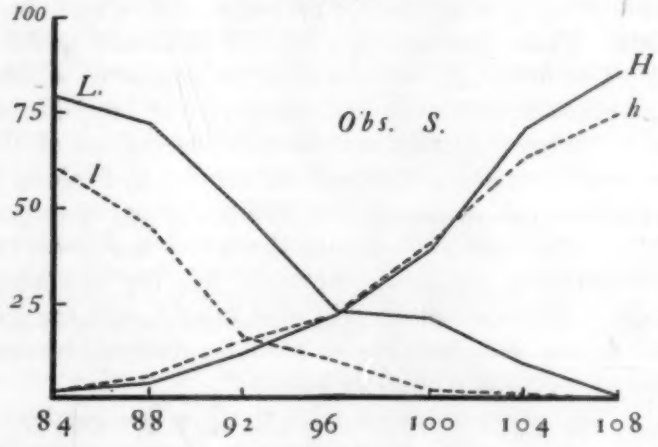

FIG. 8.

Results.—The psychometric curves are shown in Figs. 7–8. The construction of these figures follows that of the previous ones, except that for curves of the absolute judgments, symbols *l* and *h* are used to represent 'light' and 'heavy,'

respectively. For the calculated results, see Table II., the arrangement of which is the same as that of Table I., except for the change of symbols just mentioned.

TABLE II

(For explanation, see p. 481)

			C	h	$\Sigma(p'-p)^2$	IU	PSE
B	Rel.	L	88.5	.07	.002	13.1	95.3
		H	101.6	.07	.040		
	Abs.	l	85.8	.06	.003	15.4	94.8
		h	101.2	.09	.004		
S	Rel.	L	91.7	.08	.012	9.1	96.7
		H	100.8	.10	.007		
	Abs.	l	86.1	.10	.003	15.7	93.2
		h	101.8	.08	.003		

DISCUSSION OF RESULTS

A consideration of the data as shown in Figs. 1–8 brings out some striking relationships between relative and absolute methods. These relationships will be discussed under two heads. The first, (*A*), will involve the degree to which the curves approach normality and the degree of precision manifested. The second, (*B*), will include the values of *C*, the 50 per cent crossing-points of the curves, and along with them the intervals of uncertainty and the points of subjective equality. The discussion under the latter head will reveal some interesting differences between relative and absolute methods, differences which will give insight into the fundamental nature, not only of the absolute method, but of the method of constant stimuli as well.

A. The curves obtained with the absolute method, like those with the relative, appear in most cases to approximate the ogival form. For certain of the *O*s, *B*, *S*, *I*, and *W*, the curves are quite regular, for *M* and *Z* they are less so, while for *P* and *D* they are rather ragged. The smoothness or irregularity of the curves seems predominately a function of

the *O* rather than of the method. In general, if for one method an *O*s curves tend to approximate normality, those for the other method do also. It should be noted, however, that the factor of practice enters and to some extent obscures this tendency. (In *D*'s case in particular this factor may in part account for the unusual increase in smoothness of the absolute curves over the relative.)

The degree to which the two sets of curves tend to approximate normality may perhaps better be expressed in statistical terms. In Tables I. and II., column 6 shows, for each distribution, the values of the sums of the squared deviations of the obtained percentages from the theoretical ($\Sigma(p' - p)^2$).[31] An examination of these columns confirms our inference from inspection of the curves, for most of the measures indicate a fairly high approximation to normality, and the values for the absolute method are as good, if not better, than those for the relative.[32]

A consideration of the values of h, the measures of precision, as given in the 5th columns, Tables I. and II., shows that practically the same degree of precision is afforded by the two methods. It is evident that there is no general tendency for corresponding values of h to be greater for either of the two methods. This fact becomes rather significant if one allows the contention sometimes made that h (or one of its simple functions) rather than the 'difference limen' (*DL*), is a measure of the sensitivity of the subject,[33]

[31] It should be borne in mind that for a perfect fit $\Sigma(p' - p)^2$ becomes zero.

[32] The fact that most of the curves with both relative and absolute methods are practically normal justifies us in the use of Urban's process for these curves, and hence, under the assumptions of our discussion, permits a comparison of the two methods in terms of the measures which that process yields. We give the values obtained from the 'ragged' curves also, for what they are worth; they obviously are not to be taken too seriously. The reader can always go back to the raw data as shown in Figs. 1–8.

[33] The use of h as the measure of sensitivity is as old as Fechner. See 'Revision der Hauptpuncte der Psychophysik,' 1882, p. 48*f*. More recently, G. H. Thomson, in 'A new point of view in the interpretation of threshold measurements in psychophysics,' PSYCHOL. REV., 1920, 27, 300–307, has argued for the use of the interquartile range, and E. Culler, in 'Studies in psychometric theory,' PSYCHOL. MONOG., 1926, 35, No. 163, 115–122, has extolled the merits of the probable error. (Both the interquartile range and the probable error bear a simple mathematical relation to h.)

for it means that for such a determination one method is little different from the other.

B. A comparison of the two methods as regards values of *C* (column 4, Tables I. and II.) reveals an interesting fact. The '*H*' and '*3*' values (in Table II., '*H*' and '*h*') agree fairly closely, but in every instance the '*1*' (or '*l*') values are less than the 'L' values. This situation shows in the figures (Figs. 1–8), as the '*1*' (or '*l*') curves are usually shifted to the left as compared with the 'L' curves. This difference in the curves for absolute and relative methods may be accounted for if one supposes that there is, in the absolute method, (1), a spreading apart of the curves, due to a greater abundance of intermediate judgments, and (2), a shift of the curves downward in the intensity-scale, resulting from a general increase in the effective intensity of stimuli. It is apparent that for the curves of the 'heavy' judgments these two influences are opposed: the first would tend to shift the curves to the right (as the scales are shown in the figures), while the second would tend to shift them to the left, and if the two factors were of about equal effectiveness the obtained curves would show no significant change. The curves of 'light,' on the other hand, would be moved to the left by both these influences, and should show a considerable change. Actually, as mentioned above, the 'heavy' curves show no significant change, while the 'light' curves are shifted downward quite noticeably.

(1) An examination of the data shows at once that the first of the above suppositions is true; in the absolute procedure there is a much greater abundance of intermediate judgments, as is clearly shown by the greater values of the intervals of uncertainty (*IU*), given in the 7th columns of Tables I. and II.

(2) A consideration of the second supposition leads one to the measures of the points of subjective equality (*PSE*), column 8 of Tables I. and II., for it is in these that a general shift would be expected to show. Our theory is sustained in every case: for each *O* the *PSE* is lower for the absolute method. Comparison of results of the two methods is made

easier in Fig. 9, where the *PSE*s for the relative method are plotted along the upper scale, and those for the absolute method along the lower. For every *O* there is a shift to the left from relative to absolute.

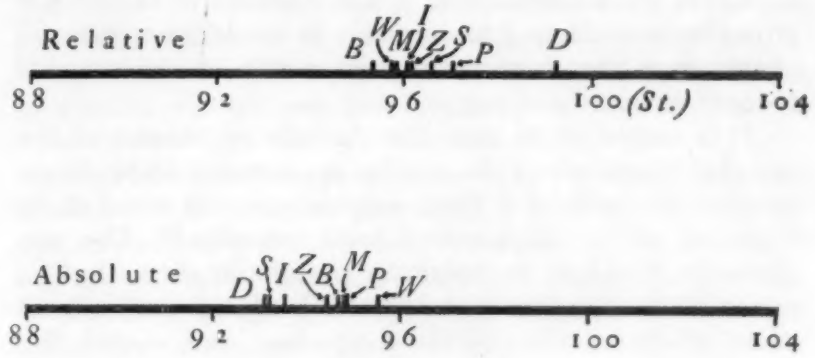

FIG. 9.

It is significant to note further that for the relative method the *PSE*s cluster about 96, which is the midpoint of the stimulus-series, while for the absolute method the *PSE*s are grouped in the region of 94. That in the absolute procedure the points of subjective equality are all below the midpoint of the stimulus-series is a very interesting fact. This situation is what we would have expected to find in the relative procedure if we had used the same series, but with the standard at 96 (the midpoint) instead of at 100. We had used the plan of shifting the standard upward, as has been customary since Urban,[34] because we thought that such a shift compensated for the time-error introduced by lifting the standard always before the comparison weight.[35] That this shifting of the standard was generally effective is quite apparent, for the *PSE*s (all except *D*'s) cluster closely about 96, reflecting the fairly symmetrical distribution of the judgments in the series. With the standard at 96, on the contrary, the *PSE*s

[34] Urban, F. M., 'The application of statistical methods to the problems of psychophysics,' 1908, p. 11.

[35] See, in regard to the time-error, Fechner, 'Elemente der Psychophysik,' 1860, I, 112*ff*., and Martin and Müller, 'Zur Analyse der Unterschiedsempfindlichkeit,' 1899, 58*ff*.

would have been shifted downward, due to a predominance of judgments of 'heavier.'

It is surprising to find, in the results of the absolute method, where no standard is present, a skewness of just the kind that the above shifting of the standard in the relative procedure was devised to correct. It would seem that the phenomenon goes beyond the mere matter of the temporal order of standard and comparison.

It is suggestive to note also that the appearance of this so-called time-error in the relative procedure is by no means confined to the field of lifted weights. Köhler found it, for example, in the judgment of tonal intensity.[36] The phenomenon he sought to explain by postulating, first, that the one stimulus, the standard, leaves behind it a certain 'sediment' which functions for the comparison; and, second, that this sediment 'settles' as time goes on, thus reducing its effective intensity, and in consequence relatively enhancing the intensity of the second tone.[37]

Moreover, Köhler suggested that the absolute impression which occurs in the relative procedure may be said to be due to the formation of a 'general level' of sediment.[38] To explain our present findings with the absolute method we need go but a step farther along the same line of theorizing; let us suppose that there occurs as the experiment progresses a gradual sinking of the general level that serves as the basis of absolute judgment. Or, in terms of our own theory, there is a constant tendency for the absolute series as a whole to move downward in the intensity-scale. Obviously, the result of any such constant tendency would be to bring each succeeding stimulus into relation with an absolute series which is shifted downward with respect to the scale of physical intensities. And as the stimulus is judged with respect to an absolute series that is 'too low,' the results, in consequence, would show a predominance of judgments of 'heavy,' as is actually the case.

[36] Köhler, W., 'Zur Theorie des Sukzessivvergleichs und der Zeitfehler,' *Psychol. Forsch.*, 1923, 4, 115–175; see also the references cited on p. 115.

[37] Idem, 166*ff*.

[38] Idem, 172*f*.

It is plain that the differences exhibited between the points of subjective equality in the two procedures are not significant in relation to the fundamental character of the methods, but are rather a consequence of an arbitrary displacement of the standard in the relative series. The one remaining difference of importance in the two methods is, then, if our above analysis is correct, the difference in the proportion of intermediate judgments. Is this difference an essential one?

We believe that the proportion of intermediate judgments made under the absolute procedure is determined chiefly by the attitude of the *O*; in the first experiment [39] we sought by instruction to regulate the factor of attitude so that the intermediate category would be on a par with the other categories. Such an attitude seemed the most natural for the method. There is every indication that this attitude was maintained in Experiment I., and in Experiment II. also, though there it was not asked for in the instructions. In consequence, the number of intermediate reports was large. We are confident that by proper instruction *O*s attitude could have been changed so as to alter the number of intermediate judgments in either direction.

Certainly one can do so in the relative procedure. The fact is notorious that in the method of constant stimuli the proportion of equal judgments is a function of the attitude of the *O*. A 'liberal' attitude occasions a large number of reports of 'equal,' while a 'conservative' or 'critical' attitude gives a smaller number.[40] Indeed, in some instances such reports occur but rarely, or not at all.[41] Because of these facts we do not believe the difference here obtained in the

[39] *Supra*, p. 478.

[40] See Washburn, M. F., 'An instance of the effect of verbal suggestion on tactual space perception,' *Amer. J. Psychol.*, 1909, 20, 447*f.*; Brown, W., 'The judgment of difference,' *Univ. Calif. Publ. Psychol.*, 1910, 1, 30–33; Fernberger, S. W., 'The effect of the attitude of the subject upon the measure of sensitivity,' *Amer. J. Psychol.*, 1914, 25, 538–543; Woodworth, R. S., 'Professor Cattell's psychophysical contributions,' *Arch. Psychol.*, 1914, 4, No. 30, 66*f.*; and Thomson, G. H., *op. cit.*, 301*f.*

[41] Brown, W., *loc. cit.*; Fernberger, S. W., 'On the elimination of the two extreme intensities of the comparison stimuli in the method of constant stimuli,' PSYCHOL. REV., 1914, 21, 353*f.*

frequencies of intermediate judgments for relative and absolute methods to be a fundamental difference between the methods; nor do we consider the problem of the control of observational attitude a more serious one in the absolute method than it is in the relative.

In any case, if one measures sensitivity in the customary manner, in terms of the 'difference limen,' the problem of attitude is a very serious one indeed. Plainly, since the magnitude of the *DL* is intrinsically dependent upon the proportion of intermediate judgments, it follows that with this criterion sensitivity is a direct function of observational attitude. This deduction, we know, runs against an assumption implicit in psychophysical determinations in general, the assumption that sensitivity is something of definite nature, basic to the organism, and expressible in fairly precise terms, and not simply a kind of shadow-shape that any wind of instruction or set can fashion to its caprice.

The situation is this, that if we are to admit sensitivity as a real entity, measurable within allowable limits of variation, we must either so carefully control attitude that its variations cause no considerable change in the *DL*,[42] or else supplant the *DL* with a new measure of sensitivity which is not so much subject to alteration by attitudinal change.

The first solution is counseled by George, who thought that much of the equivocality of the equal judgment could be eliminated by a careful restriction of what is to be reported as 'equal.' We are quite free to admit that restriction of the equal category is a great improvement, but we believe nevertheless that the desired end of constancy of measurement has by no means been obtained thereby.

We believe that in practice the above two solutions will reduce to one, for we know of but a single truly effective way to control attitude in regard to intermediate judgments, and that is so to dispose the attitude as to eliminate such judgments entirely. It is perfectly feasible; in the instruction one needs simply to state that a judgment of 'equal' is not allowed, and that report is to be made in terms only of

[42] See George, S. S., 'Attitude in relation to the psychophysical judgment,' *Amer. J. Psychol.*, 1917, 28, 1–37.

'greater' and 'less.' For obvious reasons it is advisable further to state that no two stimuli will be really equal, and that when in doubt the *O* is to guess. The remarkable fact is that the forced judgments under such circumstances are more often right than wrong.[43] *O* is seemingly demonstrating greater sensitivity than with the usual procedure. But his sensitivity as measured by the *DL* has become infinite: the *DL* has reduced to zero along with the intermediate judgments!

Of course, we could not take seriously a measure of sensitivity at infinity, but would prefer rather to call it indeterminate, and seek for a new measure. As such a measure, *h*, the measure of precision of the psychometric function, has already been suggested.[44] It is a measure of the steepness of the psychometric curve, *i.e.* it expresses the rate of change of the frequency of judgments of a certain category as a function of change of stimulus, and this, we think, is just what a measure of sensitivity ought to do. The measure seems sound in theory, but at the present time we have no further justification of its use;[45] we hope that there soon will be forthcoming experimental data sufficient to form a real test of the validity of this measure as against the traditional measure, the limen.

We seem in this discussion of fundamentals to have wandered far from the question of the properties of the absolute method. Actually, however, the method has remained in the field of our attention, for the discussion involves it as much as it does the method of constant stimuli. If one is to admit an intermediate category, and seek to control the attitude with which it is regarded, then, we think, the best procedure is to define the attitude as we have in Experiment I., for such an attitude seems a simple and natural

[43] This is admirably shown in Brown's experiment, *loc. cit.*; see especially p. 33. Cf. also, Fullerton and Cattell, 'On the perception of small differences,' especially p. 127.

[44] *Supra*, p. 485, and note 33.

[45] Culler, E. (*op. cit.*) has elaborated four 'reasons' for the use of the probable error (which ought, of course, to apply equally to *h*); these reasons, however, do not impress us as of major importance.

one, easy to maintain. But we are particularly interested here in the second type of solution mentioned above, the possibility of eliminating intermediate judgments entirely, allowing merely judgments of 'greater' and 'less,' and using as a measure of sensitivity the precision-values of the psychometric functions obtained. How constant such determinations will be, and how well they will agree with determinations by the constant-stimulus procedure with two categories remains to be discovered.

The data which we have, so far as they go, warrant us in concluding that the fundamental natures of the method of constant stimuli and the method of absolute judgment are much the same. Consequently, we are led to hold that the absolute method is a perfectly practicable method for the determination of sensitivity, a method worthy of being set alongside the method of constant stimuli for this purpose.

Summary

In this paper the method of absolute judgment has been considered, first, from the standpoint of theory; and, second, from the point of view of its use as a practical instrument for the solution of psychophysical problems.

1. The method of absolute judgment consists in the single presentation of members of a stimulus-series, and the requirement in each case of a judgment in absolute terms. Such judgment is possible through the establishment for the *O*, by the presentation of a series of stimuli, of a certain degree of knowledge of that series. The body of knowledge of the series that is adequate to absolute judgment we have called an *absolute series*.

2. Certain basic properties of the absolute series have been discussed: the manner of its establishment, its persistence, and its responsiveness to a change in the stimulus-series.

3. The psychophysical characteristics of the method of absolute judgment have been set forth by comparing the method with the method of constant stimuli in a typical experimental situation. Psychometric curves were obtained

by both methods, and it was demonstrated that the curves are significantly alike.

4. Certain differences in the results point to a difference, not in the fundamentals of the methods, but a difference of attitude affecting the frequency of intermediate judgments. This matter of attitude raises a problem of crucial importance to psychophysics in general, the problem of a proper measure of differential sensitivity. Suggestions are made for the adequate solution of this problem.

5. The method of absolute judgment is regarded, on the basis of the evidence, as a practicable method for use in psychophysical investigation.

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THE METHOD OF PAIRED COMPARISONS AS A PSYCHOMETRIC METHOD

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Professor Thurstone has opened up new fields of research in his recent articles on psychological scales and psychometric methods.[1] Problems which could not be attacked before in a quantitative way now yield to treatment by means of that most useful of scientific tools, mathematics. He also has brought unity and coherence into psychometric methods in general.

Whether or not the assumptions which he makes are logically and empirically sound is not the immediate concern of the writer. Some of them might readily be questioned from the standpoint of the psychology of judgment. Others might be questioned from the standpoint of applied mathematics. My chief concern here is to see whether his methods will give results commensurate with the labor involved.

I do not for a moment question the value of the ends which may be obtained by his methods, but I am of the opinion that the same ends may be reached by much simpler procedure. I wish to present two simpler methods which will give results comparable with those of Thurstone's and, if possible, to justify their use.

Thurstone assumes first of all that every stimulus R_k may give rise to different *discriminal processes*, S_i, S_j, S_k, S_l, S_m, etc., at different times in the same individual subject, and at the same time in different subjects.[2] The relative frequencies of these discriminal processes, S_{k-n}, obey the law of error, or are normally distributed, with the process S_k as the modal process. It is the best value (arithmetic mean) on the psychological scale corresponding to R_k on the physical or objective

[1] Thurstone, L. L., 'Psychophysical analysis,' *Amer. J. Psychol.*, 1927, **38**, 368–389.

[2] *Ibid.*, 369.

scale. By objective scale is meant a scale which is independent of the individual observer. Whereas weights or sound intensities may be measured on a physical scale, there is no physical scale for beauty, or for the affective value of colors. We can, however, assume an objective scale for beauty, and for the affective values in general, which is analogous to a physical scale of weights or of sound intensities. Thus each R_k has corresponding to it a value on the psychological scale and the psychological value has a standard error of discrimination.

When two stimuli, R_a and R_k are presented for comparison in some attribute which has been defined, *e.g.*, weight, degree of brightness, beauty, desirability, a judgment of $R_a > R_k$ or of $R_a < R_k$ will depend upon the discriminal processes of each one at the moment. The assumption that these two discriminal processes are independent, that the two stimuli do not lose their identity within a new totality, is explicitly stated.[3] This assumption might be seriously questioned, but that is not the purpose of this paper. For the present we may accept the assumption that the modal values, S_a and S_k are mutually independent in the comparison and that the scale differences between them, $(k - a)$, have a frequency distribution which also is normal.[4] This distribution will have a standard error equal to the standard error of a difference or $\sqrt{\sigma_k^2 + \sigma_a^2}$.

In the method of paired comparisons we can determine what per cent of the time each stimulus is chosen as greater than every other stimulus. From these proportions we are able to work out scale values, using Thurstone's observation equation or the 'equation of comparative judgment,' [3] which he states as follows:

$$k - a = X_{ka}\sqrt{\sigma_k^2 + \sigma_a^2} = X_{ka}\sigma_{ka},$$

in which $(k - a)$ is the distance on the psychological scale between the two discriminal processes S_k and S_a.

X_{ka} is determined from the observed per cent of judgments $R_k > R_a$, by using tables of the normal probability integral.

[3] *Ibid.*, 380.

[4] *Ibid.*, 377.

σ_{ka} is the standard deviation of the distribution of perceived differences between S_k and S_a.

In practice Thurstone has further assumed that σ_a equals σ_k, so that the equation becomes,[5]

$$k - a = X_{ka}\sqrt{2}\sigma_a,$$

and letting $\sqrt{2}\sigma_a$ become the unit of the scale, we have merely,

$$k - a = X_{ka}.$$

The last assumption, that σ_k equals σ_a may be questioned, and Thurstone has expressed his doubts concerning it in at least two places,[6] but unless we make this assumption the computations become prohibitive since σ_k and σ_a can never be directly known. This is true because we get our observations only by comparative judgments, never by absolute judgments.

The method is found in practice in a study which aims to find scale values for the seriousness of 19 crimes. The names of the crimes, such as perjury, kidnapping, abortion, arson, etc., were presented by the method of paired comparisons to 266 subjects and the per cent of times each one was chosen worse than every other one was found. By treating each stimulus in turn as a standard we can find 19 scale values for each crime. Thus, for crime *a*, we get $X_{aa} = 0$, $X_{ab} = .4593$, $X_{ac} = .4179$, etc. The final scale values are the arithmetic means of these:[7]

$$a = \frac{\Sigma X_{ka}}{N_r}; \qquad b = \frac{\Sigma X_{kb}}{N_r}; \qquad \text{etc.}$$

in which N_r is the number of stimuli.

The work is laborious, even when the discriminal dispersions are assumed to have equal standard errors. First, one must tabulate the judgments on each pair separately in

[5] Thurstone, L. L., 'The method of paired comparisons for social values,' *J. Abn. Psychol.*, 1927, 21, 390.

[6] Thurstone, L. L., 'A law of comparative judgment,' PSYCHOL. REV., 1927, 34, 282; also 'Equally often noticed differences,' *J. Educ. Psychol.*, 1927, 18, 289–293.

[7] *Ibid.*, 391.

order to find the per cent of judgments of $R_k > R_a$. The number of pairs is $N_r(N_r - 1)/2$, N_r being the number of stimuli, or 171 in the case of the 19 crimes. One must then transfer each one of these into sigma values by reference to tables and then find 19 averages. My first short-cut eliminates most of this labor. It requires a tabulation of the total number of times each stimulus was chosen, no matter in what combination it occurred, and the calculation from this of the per cent of the time it was chosen. My second method involves the translation of these per cents into sigma values. In either case we have only N_r values to deal with instead of $N_r(N_r - 1)$, or in the case of the crime data, 19 instead of 171. The details of these two procedures follow.

It was found that if we take simply the total number of times each stimulus is chosen in preference to all the rest, we will have values which bear a linear relationship to those obtained by Thurstone's method. Indeed, the Pearson coefficient of correlation between the two sets of values for the 19 crimes is .985. In another study [8] Thurstone has established scale values for the wetness or dryness of Allport's [9] thirteen proposals concerning prohibition. The aim was to find, not merely which proposals are wetter or dryer than the others but *how much* wetter or dryer. The correlation between Thurstone's scale values and the total number of preferences for each stimulus is even higher, $r = .994$. The labor of securing his scale values in the latter case has been further increased by a complicated system of weights! I do not mean to imply that the weighting scheme is inherent in Thurstone's scaling procedure. The values obtained by my own shorter method could be weighted. But what is the advantage in finding scale values by a complicated procedure which is made even more laborious by weighting, when a much simpler method without weighting gives scale values which correlate .994 with them?

What is the reason for this remarkable agreement? It

[8] Thurstone, L. L., 'The measurement of opinion,' *J. Abn. Psychol.*, 1928, 22, 415–430.

[9] Allport, F. H., and Hartman, D. A., *The American Political Science Review*, November, 1925.

lies partly in the fact that the relationship between x and y in the probability integral is almost linear for most values of y. Indeed, if y varies only between .150 and .850, the coefficient of correlation between x and y is .999. Thus, if the data contained no observed proportions less than .150 nor greater than .850, the correlation should be nearly perfect. For observed proportions of .100 and .900 the error in the scale value of each one should be in the neighborhood of 4 per cent.

But Thurstone's data on crimes contain proportions of $R_k > R_a$ as high as .989 and as low as .011. His data on prohibition contain proportions from .000 to 1.000. Yet in the latter case, there exists a coefficient of correlation of .994. I am unable to assign any other mathematical reason for this, and at least two mathematicians who have been consulted have none to offer. Empirical evidence shows that the elaborate treatment of data has given us nothing more than a unit for the psychological scale. The writer has tested this linear relationship between the observed proportions and the Thurstone scale values by using data obtained from ten color stimuli.[10] In one case (92 *Ss*), the correlation coefficient was .945, and in another, (80 *Ss*), it was .929. These lower coefficients would seem to indicate that the number of judgments obtained is a factor in the linear relationship. At present I see no logical reason why the number of judgments should be a factor.

There is another method which will give scale values in terms of sigma units and which involves few computations. We may arrive at the procedure through the logic of the *phi-gamma* hypothesis used in the method of constant stimuli. In this method it is customary to compare five or seven variable stimuli R_v, which differ by equal steps on the physical or objective scale, with a standard stimulus, R_s, which is the stimulus lying midway in the group. When we plot the proportion of judgments of $R_v > R_s$ against the stimulus values, we almost invariably get the ogive or cumulative normal

[10] I am indebted to Mr. W. E. Walton, assistant in psychology, who kindly supplied these data.

curve. In the method of paired comparisons our abscissa values are usually unknown whereas our proportions are known. Assuming an ogive distribution for the proportion of judgments $R_v > R_s$, and a value of zero for our standard, R_s, we can find the corresponding abscissa values, by referring to Fechner's fundamental tables,[11] which will give us objective scale values for the stimuli.

In using the method of paired comparisons each stimulus is used in turn as a standard. This would give us $(N_r - 1)$ scale values for each stimulus, the mean of which would be our true scale value. But this would be duplicating Thurstone's method with as much labor as before. Let us make the further assumption that all the R_v's are compared with a single standard. Let that single standard be the mean value of all the stimulus values, since each stimulus is compared with all. The exception to this statement is that in practice each R_a is never compared with itself. This can be corrected by assuming that the judgment $R_a > R_a$ will occur just 50 per cent of the time. This proportion of judgments for $R_a > R_a$ is not found empirically to be true in judgments of lifted weights and the like. Very often the 'point of subjective equality' deviates from the value of R_s in one direction or another. This could not happen, however, with most material in which the method of paired comparisons is used, for the identity of each stimulus is easily recognized. When R_a appears with R_a and their identity is recognized, a judgment of 'equal' would be the obvious thing if a judgment of 'better,' 'equal,' or 'worse,' or of some other evaluative term were asked for.

Having made these assumptions, the number of steps needed to derive scale values is reduced materially:

Step I.—Find the average per cent of times each stimulus has received a 'favored' judgment in all its comparisons. This may be found by means of the formula,

$$\text{Av. Pc.} = \frac{N_k + N_s/2}{N_s N_r}$$

[11] This table may be found in Titchener's 'Quantitative Manual,' p. 99, or in Brown and Thompson, 'Essentials of Mental Measurement,' p. 201.

in which N_k is the total number of choices given to R_k,

N_s is the number of subjects or judges,
N_r is the number of stimuli.

If in practice R_a were paired with itself, and if judgments of $R_a > R_a$ were secured, the formula would be simply

$$\text{Av. Pc.} = \frac{N_k}{N_s N_r}.$$

But this is not true in the method of paired comparisons. In order to compensate for this omission of the $R_a R_a$ pairs in practice we have assumed that the number of judgments $R_a > R_a$ is $N_s/2$, hence this amount must be added to the numerator.

Step II.—Find the corresponding scale value from Fechner's fundamental table, but preferably from Pearson's Table I.[12] The unit in the latter case will be the standard deviation of the distribution of judgments with respect to the standard, or the sigma value of the ogive curve which is assumed. If Fechner's table is used the scale values thus obtained will differ from the Pearson values by a constant factor $\sqrt{2}$. The Pearson values are 1.414 times the Fechner values.

The essential difference between this procedure and that of Thurstone is that he finds the mean of the N_r scale values *after* transferring the proportions to sigma values, whereas I have found the mean of the proportions *before* transferring them to sigma values. There is some mathematical justification for the latter procedure. According to the principle of least squares, in making any indirect observations in which the best value of $f(x)$ is to be determined from a series of observations x_i, it is better to find the mean of the x_i's *before* finding the corresponding value of $f(x)$. In the present case, the x_i's are the observed proportions.[13]

Figure 1 shows the Thurstone scale values as a function of average per cent of choices $R_k > R_a$. The best-fitting line, $y = .179x - .018$, demonstrates the linear relationship which

[12] The writer has found the best table for this purpose to be Table XII in Holzinger's 'Statistical Tables for Students in Psychology and Education,' 1925.

[13] Cf. Weld, L. D., 'Theory of Errors and Least Squares,' 1926, p. 67.

was mentioned before. The data are taken from his experiments with Allport's thirteen wet and dry proposals.[14] The zero point has been changed so as to coincide with that in my own scale values for the same thirteen proposals. The latter scale values are derived from the *phi-gamma* function which is also represented.

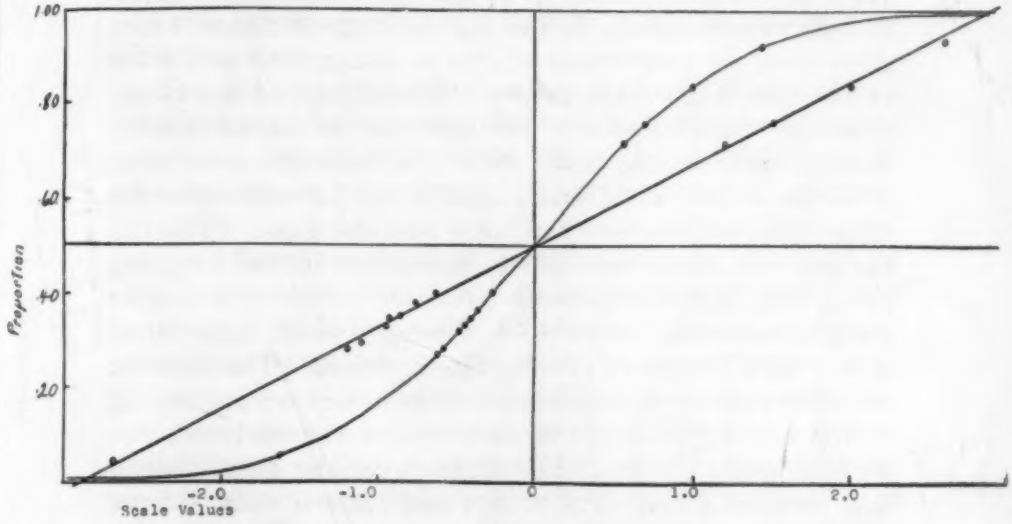

FIG. 1. The abscissa represents scale values in sigma units, the zero point being chosen at the mean of the scale values which are represented on the curves. The ordinate represents the observed proportions of judgments $R_a > R_k$. The straight line passes through Thurstone's proportions and scale values; the ogive is the function from which my own scale values are derived on the assumption that the *phi-gamma* hypothesis holds for judgments of these stimuli as well as for judgments of lifted weights and the like.

There is one difficulty which is encountered in Thurstone's procedure but not in my own. Should it happen, as it did a number of times in the problem just referred to, that the proportion of judgments $R_k > R_a$ is 1.000 (or .000), the corresponding scale difference, $(k - a)$, is infinite. The mean of the scale values, using every stimulus as standard, then becomes indeterminate. If, however, we secure a mean value of the observed proportions, this mean will never be as great as 1.000 nor as small as .000 because the proportion of .500

[14] Thurstone, L. L., *J. Abn. Psychol.*, 1928, 22, 421.

will always occur at least once for each stimulus, from the case $R_a > R_a$.

But the latter procedure involves us in an apparent dilemma. The procedure is based upon Urban's method of constant stimuli, in which there is a single standard stimulus and the variable stimuli are grouped fairly closely about it. With the limited number of judgments which can be given to each pair of stimuli, R_s and R_v, the range of stimuli which gives observed proportions of .000 at one extreme and 1.000 at the other is relatively narrow. Stimuli beyond this range, according to Thurstone's assumptions, lie at an infinite distance from the standard. Now if we make the mean value of all the stimuli our standard, there will be no limit to the range lying between proportions of .000 and 1.000. This has the practical value, which was mentioned in the foregoing paragraph, that stimuli very far apart in the scale can be given determinate values. On the other hand, it is inconsistent with Thurstone's original assumptions. The dilemma will disappear if we remember that in theory we are dealing with a very large number of observations whereas in practice we have relatively few. The range of variable stimuli which falls between proportions of .000 and 1.000 is really infinite even with a single stimulus as standard. With a large enough number of observations the proportions of .000 and 1.000 would merely be approximated, never reached.

Nevertheless, the assumption of the mean value as the standard results in a bunching together of the scale values. In my own procedure this is due to the fact that the standard is not a single stimulus but a mean of several stimuli, including the variable itself. In every case the mean proportion is derived from a series of proportions one of which is .500. There is a similar bunching effect, although to a less extent, with Thurstone's procedure, due to the fact that he also has secured a mean scale value and every crime when compared with itself as standard is given a scale value of zero. Nor is this bunching effect a constant affair, for his procedure or for my own. The smaller the number of stimuli, the greater is the bunching, since with fewer stimuli the proportion of .500,

from the assumed judgments $R_a > R_a$, has relatively more weight. This may be logically sound in that the greatest of twenty stimuli would be expected to deviate farther from the average than would the greatest of ten. But it would not be correct to compare two different continua as to their extent unless the standard contained the same number of stimuli. This could be made a constant factor in every continuum by selecting ten representative stimuli, for example, as the combined standard.

The selection of a given number of stimuli as a combined standard would also materially reduce the number of comparative judgments to be made. It would probably never be wise to reduce the number of standard stimuli to one, especially for judgments which involve feeling to a great degree. From the point of view of statistics the errors of sampling would be too great. From the point of view of the psychology of feeling, adaptation would be an important disturbing factor. From the wide variation in the scale distances $(k - a)$ as derived from different single standards—and these variations may be observed in Thurstone's data as well as in my own—one suspects that for each pair of stimuli new criteria may enter. In order to insure uniformity of criteria, and hence a single, linear continuum, each stimulus ought to be compared with a number of other stimuli in that continuum. For example, I might prefer R_x to R_y for one reason, but in general when these two are compared with other standards I prefer R_y more often than I do R_x for reasons not appearing in the R_xR_y comparison. And again, if R_a were compared only with R_b, R_b with R_c, R_c with R_d, and so on, there is no guaranteeing a single dimension in the resulting scale values.

I have assumed in adopting the *phi-gamma* hypothesis for my data, that the abscissa units, as in all psychophysical work, are on a physical continuum. By a very similar procedure Thurstone has secured scale values which he assumes are on a psychological continuum. The question now arises, are the units in both cases psychological, are they objective stimulus-values, or are they psychological in the one

procedure and objective in the other? If the symmetrical ogive curve is the rule in the method of constant stimuli, the units must be physical or objective in both cases, since in both the *phi-gamma* function has been used. The only difference is that Thurstone has secured a mean of values on the abscissa whereas I have obtained a mean of values on the ordinate.

There is one indication that the abscissa is a psychological continuum. Lufkin [15] has found that Urban's data is nearly always best fitted by Pearson's curve of Type I., which is skewed in the negative direction for judgments of 'greater.' The corresponding integral function will be asymmetrical, with a greater precision on the negative side. It is not inconceivable that this skewness is due to the Fechnerian relationship between stimulus difference and observed difference. If the Fechnerian law holds between every psychological continuum and its corresponding objective continuum, even when the psychological continuum is one of feeling, then the symmetrical ogive may obtain when we have psychological units on the abscissa and a skewed curve when the units are physical or objective. If this were true, our treatment of data in the method of paired comparisons would give us scale values in psychological units. It is doubtful, however, whether the skewness is so great that we need to invoke the Fechnerian law to account for it. Lufkin finds that Urban's data fit the normal curve almost as well as they do the skewed curve.[16] We shall have to let experiment settle this point.

From a comparison of the two curves in Fig. 1, it will be seen that my scale values have an ogive relationship with those of Thurstone. This relationship is necessarily true because his scale values have a linear relationship which correlates approximately plus 1.000 with the means of the observed proportions themselves, whereas the latter have the assumed ogive relationship to my own scale values. In psychophysical work the observed proportions have an ogive

[15] Lufkin, H. M., 'The best fitting frequency function for Urban's lifted-weight results,' *Amer. J. Psychol.*, 1928, 40, 80.

[16] *Ibid.*, 81.

relationship to the physical scale on which the stimuli are measured. From this it appears that he is correct, that his scale values are on a psychological continuum, while mine, it would seem, are on an objective continuum. This also signifies that the means of the observed proportions, if their linear relationship to Thurstone's scale values is valid, are also on the psychological continuum.

If, then, one wishes a scale of psychological values, and if one is not particular about what unit he uses, one need merely take the means of the observed proportions, or the per cent of the time each stimulus is preferred in all its comparisons. This short-cut must be used with one caution. It has been empirically but not mathematically justified. Thurstone,[17] in a communication to me, has stated that deviations from linearity are found near the extremes in the scale in some data yet unpublished. If this is true of all scales which extend far enough, there is no mathematical proof for linearity. But empirical evidence lends reliability to this short-cut for scales which do not extend too far. The limits within which the short-cut is reliable will also have to be determined empirically but it is probably safe to use the short-cut if the mean proportions lie between .100 and .900. No doubt this will cover a majority of the cases in which a scaling procedure is needed.

In most quantitative work, however, we are accustomed to work with stimulus values. My second procedure may be used to determine stimulus values for which there are no physical determinations. In all psychophysical work we are concerned with just the two variables; the stimulus magnitude and the psychological consequence of this magnitude which is measured by the proportion of favorable judgments given that magnitude. If the stimulus magnitude can be defined in the more refined terms of physical measurement, so much the better; if not, we may determine it indirectly by applying the method of paired comparisons and treating the data according to the method of constant stimuli reversed. That

[17] I am greatly indebted to Professor Thurstone for this and for other suggestions and criticisms, some of which I have taken advantage of in this paper.

is, we may find the stimulus magnitude from the observed proportions via the *phi-gamma* function.

Summary

This article summarizes Thurstone's statistical procedure for computing scale values of stimuli presented by the method of paired comparisons, and the statistical theory which is involved. Thurstone assumes a psychological continuum upon which these values lie.

A short-cut is presented by which one may find scale values that correlate almost perfectly with values found by means of Thurstone's more elaborate method.

A method is suggested for getting scale values which are assumed to lie on an 'objective' continuum. These scale values are in terms of sigma units from an assumed mean of all the stimulus values.

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AUDITORY PERSISTENCE, SUMMATION, AND FUSION IN SUCCESSIVE IMPULSE-PERIODS

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Ohm's Law of Hearing states that in attending to a complex sound, tones can be heard corresponding to the simple harmonic components which the Fourier analysis reveals. This, together with the fact that, physically, a synthetic wave is the sum of a number of simple vibratory motions, led Helmholtz and subsequent investigators to consider the hearing mechanism as an analytical device for breaking up the compounded tone into its single simultaneously-sounding components, or of reinstating a hypothetical physical condition in which there are a number of independent systems of vibratory movements.

But the hearing of certain tones from a synthetic sound which were not present in the group of original components, such as beat-tones and combination-tones of other kinds, has led all investigators to look in part, at least, to the nature of the compounded or resultant wave itself for an explanation of the hearing of these additive tones. A number of theorists have withdrawn entirely from a Helmholtzian analytical theory of hearing to a theory which would explain all tones heard on the nature of the complex wave itself.

Certain preliminary experiments with siren discs indicate that the lead which this latter class of theorists are following may be entirely justified; that 'timbre-quality,' [1] and pitch-

[1] The term 'quality' is here used in the sense of 'character' since there are many auditory qualities depending upon the nature of the temporal pattern of sound waves such as, for example, are included under Metfessel's concept of *sonance* (*Cf.* PSYCHOL. REV., 1926, 33, 459-466). The term 'timbre' has generally been confined to the fusion of periods within a single complex wave, whereas there are many other qualities resulting from the blending of successive waves. To be exact, however, all fusion and sound blends are the resultant of successive excitation of the auditory mechanism, even within a single complex wave.

complexes of all kinds are the resultant of successive fusion of continuous or discontinuous impulse-periods effected through the persistence of auditory excitation and a process of summation.

In a circle on a large disc there were punched a pair of holes each of 4 mm. diameter with a space between them corresponding to 1/72 of the circumference. In a second concentric circle, 7 cm. inwards, there were punched 72 holes, also of 4 mm. diameter, each separated by a space 1/72 of the circumference. When this disc was rotated and the outer pair of holes was caused to cut a blast of air but once, a single discrete sound was heard possessing a definite pitch-quality which was readily verifiable by checking against the pitch heard when the inner circle of holes was caused to cut the blast of air.[2] Designating the distance or time interval between a pair of holes as an 'impulse-period,' it has been shown that a single impulse-period is sufficient to produce the hearing of pitch-quality which is definite.[3]

[2] The writer has repeatedly verified this and other conclusions herein reported by the observations of a number of good observers within the limits of periods corresponding to frequency rates of 40 d.v. to 1,088 d.v. For a verification of the sound produced from a complex pattern of different successive periods (see later discussion), a metal disc was used, rotated through a magnetic field connected to a good telephone receiver, thus eliminating the noise produced by the cutting of the blast of air, and reducing the action of the air to the external ear.

[3] This is in accord with the presumptions created from the work of Savart (1830), Villari and Marangoni (1868), Pfaundler (1878), Cross and Maltby (1891), Abraham and Brühl (1898), Gianfranceschi (1914), Stefanini (1917), Abraham (1920), and Kucharski (1923) with reference to the number of vibrations necessary to produce pitch-quality. In those experiments which concluded that a fraction of one 'vibration' was sufficient to result in the sensation of tone, it is not unreasonable to suppose, on the basis of the known action of elastic media, that an 'impulse-period' in the sense here used was created, for physically vibrations cannot be fractionated. The conclusions of the present writer do not agree with those of Abraham (*Annalen der Physik.*, 1919, **60**, p. 59) which state that the sound produced from a pair of holes, such as the present writer has used, is qualitatively undifferentiated from the clap-like noise emerging from a single hole. Moreover Abraham reports that the pitch from the two holes is very difficult to ascertain, a conclusion not in exact accord with the repeated finding reported above. If a disc be cut with two concentric circles upon which are punched a pair of holes and a single hole respectively, and the disc be caused to rotate at various rates, the pitch-quality from the two holes can be heard to change markedly with change in speed of rotation, whereas the quality of the sound produced from the single hole does not change materially; the change which does result is not discernible except when large differences in rate of rotation are contrasted, and then the difference is due to differences in the period of the blast itself.

Utilizing a siren disc upon which were constructed concentric circles having one, two, three, four, five, six, seven, and eight impulse-periods respectively, corresponding to 1/36 of the circumference, it was established that in sounds of short duration in which there is a recurrence of a particular period, the larger the number of consecutively recurring periods, other things remaining the same, the more tonal or the more dominant is the pitch-quality, the longer the duration, and the louder is the sound up to a certain point beyond which a law of diminishing effect is operative, except of course for the factor of duration. The point at which this law of diminishing effect for tonality and loudness operates in sounds of consecutively recurring periods varies from 2 to 5 periods within the frequency rates investigated.[4]

When consideration is given to sustained tones, it has been demonstrated that the sensation of continuous uninterrupted tone is not dependent upon a consecutive recurrence of an impulse-period;[5] gaps or intervals which are multiples of the period may occur between the impulse-period and its recurrence while the sensation of tone of the same pitch-quality is still received in somewhat[6] the same manner as though the period recurred consecutively. Thus, for example, it has been shown that a period of 1/180 of a

[4] This is in accord with the general statement of Abraham (*Annalen der Physik.*, 1919, **60**, p. 59) with reference to the increasing strength of the tone, although Abraham did not quantify the result. See also Abraham and Brühl (*Zsch. f. Psychol.*, 1898, **18**, 177–217); Exner (*Arch. f. d. ges. Physiol.*, 1876, 13, p. 234); and Urbantschitsch (*Arch. f. d. ges. Physiol.*, 1881, 25, p. 323). These latter give radically higher values due to the technique used.

[5] Wrightson ('An Enquiry into the Analytical Mechanism of the Internal Ear,' 1918, pp. 31 and 32) demonstrated that gaps which are fractions of the period do not impair the sensation of tone. This has been verified by the present writer for a wide range of situations and is a condition *sine qua non* for the building of chords from recurring patterns of differing periods. The effect of differences in the size of the gap or interval upon the tone heard has not precisely been investigated, although there seems to be a varying effect.

Meyer, M. (*Zsch. f. Psychol.*, 1896, **11**, 210–214) utilized a disc which showed a condition similar to that suggested above. Likewise his more complex patterns, comparable to the work which has been done on the interruption-tone and intermittent-tone, tends to support this thesis (*loc. cit.*, 201–205).

[6] This refers (1) to the summation effect in intensity of recurring periods and its converse; and (2) to the production of new tones as described in a subsequent paragraph.

second (frequency rate of 180 d.v.) need recur at a rate of but twenty times per second to produce the sensation of a continuous uninterrupted tone of a frequency rate of 180 d.v., and this has not been ascertained as a maximum limit. In such a demonstration there is an exemplification of (1) the persistence of excitation of a rather high order within the hearing mechanism which effects (2) the fusion of discontinuous periods.[7]

In a number of experiments it has been shown that the interval possible between a period and its recurrence, in the hearing of a continuous tone, varies with the period itself; the smaller the period, the relatively and absolutely longer may be the interval between the recurrence of impulse-periods.

In the siren disc on which there was constructed a gap or interval between an impulse-period and its recurrence, it was discovered that this interval in itself constituted a period of its own which, upon regular recurrence, produced the sensation of a continuous tone corresponding to its period.[8] Thus it may be stated as an effective working principle that the regular (although not necessarily consecutive) recurrence of any period, within certain limits of hearing, produces the sensation of continuous tone. By an extension of this principle, there may be constructed on siren discs complex recurring patterns of different successive periods in which tones are heard corresponding to each of the constituent periods, the period of recurrence of the complete pattern itself being considered as a 'fundamental' [9] period, the smaller constituent periods considered as secondary periods. For

[7] This is not in agreement with the general conclusion of Kucharski, P. (*C. r. soc. biol.*, 1927, **97**, 191–193) which stated that the abatement of sensory excitation in audition was extremely rapid and that fusion of discontinuous excitation could not be produced. This was based upon experiments with the interruption tone produced by cutting a tuning fork tone. But in experiments with the so-called interruption tone employing siren discs, there is no lack of agreement with the conclusion of the present writer; rather is there exact agreement as the present writer has verified and reported in a later footnote.

[8] *Cf.* Meyer, M., *op. cit.*, 210–214.

[9] The term 'fundamental' refers to the lowest tone heard and does not imply the loudest or most dominantly sensed tone, for in many patterns the highest tone is most dominant.

example, if two successive periods of the order of 1/30 and 1/45 of the circumference recur regularly so that the pattern of two successive periods occurs within a period of 1/18 of the circumference or eighteen times in one revolution, we hear three tones corresponding to frequency rates of 180 d.v., 270 d.v., and 108 d.v., when a blast of air is cut by the disc rotating at a rate of six revolutions per second.[10] In the same manner chords of six identifiable tones have been built up by the recurrence of successively differing periods. Thus large differences in 'timbre-quality' can be built up with the same 'fundamental' period; chords can be produced consisting of a large number of smooth and continuous tones; two klangs which have been rated as highly dissonant can be produced without the usually perceived roughness; and sound-complexes of other known kinds can be built up by successively differing periods.

In the production of 'complex tones' or 'pitch-complexes' on this basis, it is extraneous to consider that each period is recurring consecutively or that this complex tone is compounded from a number of continuously and simultaneously sounding partials.

[10] The experimentation of previous investigators is readily interpretable in the light of the principles thus far presented. *Cf.* E. W. Scripture's interpretation of Fletcher's experiment (*Zsch. f. Sinnesphysiol.*, 1927, **58**, 195–208) in which the same 'fundamental' tone was present even after interference tubes had supposedly eliminated the lower frequencies, due to the recurrence of a group of so-called higher frequencies. The 'fundamental' tone in Lachmund's sirens (*Zsch. f. Psychol.*, 1922, **88**, 1–53) was produced by the period of recurrence of the entire pattern. In addition, there is heard a complex tone which is capable of analysis into tones corresponding to each of the constituent periods. This latter conclusion is presented by the present writer from the construction of patterns according to Lachmund's description, and is in accord with the principles presented in this paper. Lachmund's 'derangement' in the regularity of progression of the sound wave series was nothing more than the construction of various 'timbre-qualities' according to various recurring patterns of different consecutive periods.

The work of Dennert (*Arch. f. Ohrenheilkunde*, 1887, **24**, 181), Hermann (*Arch. f. d. ges. Physiol.*, 1894, **56**, 490–493), Meyer, M. (*op. cit.*), Schaefer and Abraham (*Arch. f. d. ges. Physiol.*, 1901, **83**, 207–211) on the so-called interruption tone fits into the synthesis of findings in this paper although in certain instances the patterns created are relatively much more complex. Whenever a periodic blocking of holes on a siren disc is made, three tones are heard corresponding to the original period of the holes, the period created by the blocking, and the period created by the interval between the beginning of one blocked period and the next, the relative loudness of each tone depending upon a number of conditions which can be stated.

It has also been discovered that the longer the interval between the recurrence of an impulse-period, the less intense or dominant does the tone from this period become in relation to lower tones produced from the longer intervals which separate it; or positively, the higher the rate of recurrence of an impulse-period, other things remaining constant, the relatively more intense and dominant is the tone from this period. From such a conclusion it would seem that there is a process of summation in the hearing mechanism such that the rate of recurrence of a period produces changes in the intensity of the tone heard outside of the intensity changes due to amplitude differences of the impulse.

The known facts with reference to the production of roughness or noisiness apply in a complex situation. When an impulse-period recurs at irregular intervals, a noisiness is heard, behind which is perceived a continuous uninterrupted tone corresponding to the recurring period.[11] The irregularity in the size of the intervals between the recurrence of a particular period produces the noisiness.[12]

Thus far differences in impulse-periods where the amplitude of the impulses remained constant have been considered. Attention may now be directed to the results of experiments with siren discs utilizing differences in the size of holes to produce differences in intensity and wave-form.[13]

A disc was constructed in which consecutive periods of 1/36 of the circumference recurred, thus giving equal periods or identical periods throughout the circumference. The first two holes on the circle were made 2 mm. in diameter, the third hole 7 mm., the next two holes 2 mm., the third 7 mm. This pattern of size was repeated throughout the entire circumference thus giving twelve holes of 7 mm. diameter recurring at regular intervals so as to form a period between them

[11] This is a verification of Wrightson's conclusion (*op. cit.*) which is of significance in the understanding of certain sound complexes.

[12] Irregular differences in amplitude and wave-form produce the sensation of noisiness in accord with the same principle and are explainable on the basis of irregularity in the periods which intensity-differences and wave-form-differences create.

[13] In siren discs, especially, it is impossible to divorce form of impulse entirely from amplitude. These must be considered together, with preference given to an acceptance of an amplitude effect rather than a wave-form effect.

corresponding to 1/12 of the circumference. When a blast of air was cut by this disc rotating at a rate of eight times per second, two tones were heard corresponding to frequency rates of 288 d.v. and 96 d.v. From the results of this experiment it may be stated that a recurring difference in intensity creates a period of its own giving rise to the sensation of a tone corresponding to this period.[14]

Upon the same disc a circle of holes with the same period, namely of 1/36 the circumference, was cut with a pattern of diameters of holes as follows: the first two holes were cut 2 mm. in diameter, the third 7 mm., the next five holes 2 mm., the ninth 7 mm. This pattern was repeated throughout the circumference. There were created three periods corresponding to 1/36, 1/12, and 1/6 of the circumference respectively, and when this circle of holes, rotating at a rate of eight revolutions per second, cut a blast of air, three tones were heard corresponding to these periods or of frequency rates of 288 d.v., 96 d.v., and 48 d.v. There is here an extension of a previous principle. The sensation of a continuous, smooth, uninterrupted tone from periods created by intensity differences is not dependent upon a continuous recurrence of these periods; gaps or intensity intervals may occur between a period and its recurrence and the sensation of tone is still heard, although with diminished relative intensity. Noisiness is demonstrable in the same manner for periods created by intensity differences as previously.

As a final experiment in this series, a disc was cut with recurrent periods corresponding to 1/36 of the circumference of the circle with differences in the size of holes according to the following pattern: the first hole was 2 mm. in diameter, the second 4 mm., the next three 2 mm., the sixth 7 mm. This pattern was repeated throughout the circumference. There were created from this pattern, four periods corresponding to 1/36, 1/18, 1/9, and 1/6 of the circumference

[14] This is in accord with the finding of Schaefer and Abraham (*op. cit.*). Beat-tones are explainable in the same manner. The construction of a pattern similar to a beat-tone has been reported by R. Koenig ('Quelques expériences d'acoustique,' 1882, p. 140) the results of experimentation with it agreeing with the above principle. Schaefer and Abraham, and the present writer have verified his conclusion.

respectively. When the disc, rotating at a rate of eight revolutions per second, cut a blast of air, a smoothly-sounding chord was heard in which tones approximating frequency rates of 288 d.v., 144 d.v., 72 d.v., and 48 d.v. respectively were identified. Thus it is seen that effective periods are created where there is a recurrence of periods which have intensity effects in common. A period may be created between an impulse of small intensity and an impulse of larger intensity.

On the basis of the facts experimentally derived and presented in this paper, resultant complex waves from all sorts of partials can be simulated through variations in the periods or intervals between holes and intensity differences among the impulses; the laws of hearing from these various patterns can be derived and stated without reference to any assumed simultaneously recurring components.

Summary

This paper has presented a synopsis of principal findings from a series of experiments with siren discs accurately constructed and utilizing a blast of air for sound production. Observers with high auditory acuity and pitch discrimination reported tones heard by means of vocal reproduction and matching with a tone variator. Comparison was made between the periods of these reported tones and the known periods of the sound produced from the siren discs.

It has been pointed out that in sustained sounds there is (1) a persistence of auditory excitation which effects the fusion of discontinuous periods; (2) a summation effect of recurring impulse-periods which results in perceived intensity differences according to the rate of recurrence; (3) a fusion of successively differing impulse-periods effecting the hearing of complex sounds of various kinds; and (4) a period effective for the hearing of a tone from impulses which have intensity effects in common.

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THE THEORY OF SOUND LOCALIZATION: A RESTATEMENT

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Introduction.—Research in the field of sound localization [1] is concerned, at the present time, not so much with the exploration and mapping of auditory space, but rather with the nature and specificity of the stimuli that result in localization. Just now, the temporal factor, perhaps because its function has so recently been defined, is receiving most emphasis; and phase, because it is presumably a special case of the temporal factor, is receiving much attention. Interest in the intensive factor persists because investigators have suggested that phase and time are reducible to intensity. Complexity, or mass, the physical correlate of quality, will doubtless receive more emphasis in the future, as those of the school of *Gestalt* proceed in their study of the configurational nature of auditory space.

Experimentation has repeatedly demonstrated that each of the above factors is effective as a physical variant when the others are controlled, in determining directional localization of sound. But, does one of these factors predominate in the complex conditions of ordinary hearing; are they all reducible to one; or do they each and all contribute to a pattern that results in directional perception? A brief statement of the general facts of localization as conditioned by each of the physical factors will doubtless throw some light on this question.

The Binaural Ratio.[2]—(1) Binaural differences in intensity result in what has been called the 'intensity-effect,' *i.e.* the

[1] A synopsis of recent literature on sound localization is given by Ruckmick, C. A., 'Recent acoustic research,' PSYCHOL. BULL., 1924, **21**, 617–621; also, 'Auditory sensations and related phenomena,' *ibid.*, 1927, **24**, 89–91.

[2] The following review the literature on the binaural ratio: Pierce, A. H., 'Studies in space perception,' N. Y., 1901; Ferree, C. E. and Collins, R., 'An experimental

rotation of the phantom sound about the head. When two tones of the same frequency and intensity are presented, one at each ear, in such a way that like phases arrive simultaneously, a single fused tone is localized in the median plane. If the intensive relationship is gradually altered so that, in effect, one of the tones gradually becomes relatively stronger, the apparent source of the tone is gradually displaced from the median plane, through an arc of fairly constant radius, to the aural axis on the side of the ear which receives the more intense stimulus.

(2) The vertical angle of the transverse plane of the arc described as the 'intensity-effect' is not a function of the binaural ratio; it apparently depends upon the method of observation.

(3) The relationship between intensive differences and angular displacement is linear; and angular displacement, θ can be expressed in terms of the right and left intensities, I_r and I_e respectively in the following formula:

$$\theta = K \log \frac{I_r}{I_e},$$

in which K is constant for an individual.

(4) The binaural 'intensity-effect' does not obtain throughout the tonal range, for all individuals of normal acuity.

(5) The intensity-thresholds of localization have not been determined in absolute units; but (*a*) a relatively large change in intensity is necessary to produce displacement in localization; and (*b*) if the difference in the intensities is relatively large, provided the absolute intensities are also relatively large, two phantom sources are perceived, one in a stationary position in the median plane while the other rotates about

demonstration of the binaural ratio as a factor in auditory localization,' *Amer. J. Psychol.*, 1911, 22, 250–297; Stewart, G. W., and Hovda, O., 'The intensity factor in binaural localization; an extension of Weber's law,' PSYCHOL. REV. 1918, 25, 242–251; Stewart, G. W., 'The intensity logarithmic law and the difference of phase effect in binaural audition,' PSYCHOL. MONOG., 1922, 31 (1), 30–44; Banister, H., 'Three experiments on the localization of tones,' *Brit. J. Psychol.*, 1926, 16, 265–292; Halverson, H. M., 'Binaural localization of tones as dependent upon differences of phase and intensity,' *Amer. J. Psychol.*, 1922, 33, 178–212 (referred to hereafter as Halverson, 1st).

the head; or they both may remain stationary at the extreme lateral positions.

(6) The very slight differences in intensity that result in the 'intensity-effect' are not perceived as intensive differences as such; they are heard as direction.

The Phase Difference.[3]—(1) With binaural differences in phase, the phantom sound rotates about the head. This has been called the 'phase effect.' If like phases of a given frequency are presented simultaneously one at each ear, when the intensities of the stimulating tones are kept equal and constant, a single fused tone is localized in the median plane. When the phase leads to either side, the phantom source shifts to that side and describes an arc from the median plane to the aural axis as the leading phase increases to exactly one half wave length, at which time the two tones are again in phase. Further increase in lead after this point changes the localization to the other side, and the course is described in reverse order, the phantom coming again to the median plane when the leading tone is a wave-length ahead of the other.

(2) Differences in phase do not determine the angle of the transverse plane of the arc described as the 'phase effect.' This angle is presumably dependent upon auditory habits peculiar to the individual.

(3) The relationship between phase differences and angular displacement is approximately linear.

(4) The 'phase effect' does not hold throughout the tonal range. If the tone is low, below approximately 200 d.v., phase differences are only partially effective. The upper limit of the frequency range of 'phase effect' has been placed at various frequencies ranging from 512 to 17,000 d.v.

(5) The phase thresholds of localization have not been defined; but (*a*) tones must be out of phase somewhat for an appreciable displacement of the phantom sound, and (*b*)

[3] The following have summarized the literature on 'phase-effect': Stewart, G. W., 'Binaural beats,' *Phys. Rev.*, 1917, **9** (2), 502–508; Seashore, C. E., 'Wave phase in the open air localization of sound,' PSYCHOL. MONOG., 1922, **31** (1), 1–6; Halverson, *op. cit.*; Halverson, H. M., 'The upper limit of auditory localization,' *ibid.*, 1927, **38**, 97–106; and Banister, *op. cit.*

when the phase difference extends above approximately 2.5σ in point of time, bilateral localization results.

(6) Phase differences are not directly perceived.

The Temporal Difference.[4]—(1) With binaural differences in time, the fused sound rotates about the head. This may be called the 'temporal effect.' When two discrete sounds that are qualitatively alike and equally intense are presented simultaneously one at each ear a single fused sound is localized in the median plane. If the sounds are exposed so that one is prior to the other by a gradually increasing interval of time, the fused sound [5] gradually shifts from the median plane to the aural axis on the side of the ear first stimulated.

(2) The vertical angle of the transverse plane of the arc of 'time-effect' is not determined by the temporal difference of arrival of the stimuli.

(3) The relationship between angular displacement and temporal differences is approximately rectilinear.

(4) Since the 'time-effect' is better defined when relatively high-pitched sounds are used as stimuli, it seems that this 'effect' may be a function of frequency.

(5) The lower temporal threshold of localization has been variously defined, as 2$\sigma\sigma$, 30$\sigma\sigma$, .06σ, .1σ; while the upper limit has been placed at various intervals ranging from .63σ to 16.5σ.

(6) The very slight differences in time that result in what has been called the 'temporal effect' are not perceived as temporal differences as such.

It has been demonstrated [6] that phase, time, and intensity

[4] See: Wittmann, J., 'Beiträge zur Analyse des Hörens bei Dicotischer Reizaufnahme,' *Arch. f. d. ges. Psychol.*, 1925, **51**, 21–122; and Trimble, O. C., 'Some temporal aspects of sound localization,' *Univ. Iowa Studies in Psychol.*, 1928, **11** 172–225.

[5] A second phantom sound is localized symmetrically opposite the fused sound. But this sound is accounted for on the basis of the diffraction of the stimulating sounds about the head, the prior sound arriving at the opposite ear before the later sound arrives at the ear first stimulated. This phantom then is but another example of the effectiveness of the temporal factor.

[6] Klemm, O., 'Über den Einfluss des binauralen Zeitunterschieds auf die Lokalisation,' *Arch. f. d. ges. Psychol.*, 1920, **40**, 117–146. Wittmann, *op. cit.* Halverson, *op. cit.*, 1st.

may supplement, or neutralize, each other to a varying degree in their effect upon localization.

The Rôle of Complexity.[7]—(1) A 'mass-difference effect' comparable with the 'effects' described above has not been experimentally determined. In the case of complex standing waves, each tone has its own phantom source which rotates in its own orbit at its own peculiar rate; but just what the effect would be of gradually increasing the complexity of one of the stimulating sounds with the other held constant has not been demonstrated.

(2) Complex sounds are more readily and more accurately localized than more simple sounds.

This review of the facts of directional localization shows that phase, time, and intensity function in much the same manner. Their 'effects' are alike; the relationship of the angular displacement to the differences in the stimuli in each case is approximately the same; each factor has its optimal effectiveness; the very slight differences in the stimuli in each case are not perceived as differences in phase, time, and intensity as such; and each may modify the effectiveness of the others. But these facts were obtained under conditions which seldom, if ever, occur in ordinary hearing. The factor of complexity, which results in more precise and immediate localization, and which doubtless involves an unequal binaural distribution of more than one of the above factors, represents conditions which approximate those of normal hearing. Thus, the question set forth above has been more clearly defined by indicating its sources and implications. *In a complex of sound, does either phase, time, or intensity predominate in determining the directional localization; or are all of these factors reducible to some common factor; or do they each and all contribute to a pattern of effects that results in directional perception?*

Theoretical Interpretations.—This question has been variously answered. Klemm [8] suggests that a temporal difference may be reducible to an intensive difference because of inter-

[7] See: Ferree and Collins, *op. cit.*, Seashore, *op. cit.*, Halverson, *op. cit.*, 1st.

[8] Klemm, *op. cit.*

ference in the later stimulated ear, due either to bone-conduction or to diffraction of sound about the head. While he leans toward this interpretation he admits the insurmountable difficulties of the problem of bone-conduction and the question of the effectiveness of very slight intensive differences. Klemm also suggests that a temporal difference may in reality be a phase difference because telephone receivers such as he used may not be 'dead-beat'; or that phase differences may result from continued vibratory movements of the cochlear fluids; or that temporal differences may be reduced to phase differences in the perseveration of the neural currents over the auditory tracts. He discounts this interpretation, however, on the basis that the phenomena of localization should change periodically with increasing temporal difference and with changes in the pitch of the stimulating sounds, and concludes that the sound impressions, however created, result in 'movement' along the auditory pathway from ear to ear; and that a localization is but an almost 'congealed motion.'

Hornbostel, and Wertheimer [9] define phase-difference as a special case of time-difference, deny the adequacy of intensity as an agent in localization, and hold that localization can be completely accounted for in terms of the temporal difference of arrival of sound at the ears. They suggest that the physiological processes which form the basis of directional perception are central rather than peripheral.

Boring [10] holds that phase-difference is but a special case of time-difference, just as time-difference is but a special case of intensity-difference. He claims that because of inhibitory processes, the cortical excitation is greater for the prior stimulus, and that a temporal, or phase, difference is therefore reduced to an intensive difference at the auditory center; *i.e.*, the prior stimulus to a greater or less degree, according to the temporal interval, or phase lead, inhibits the later. The physiology of such inhibition, Boring admits, is

[9] Hornbostel, E. M. v. and Wertheimer, M., 'Über der Wahrnehmung der Schallrichtung,' *Sitzungsb. d. Preuss. Akad. d. Wissensch.*, 1920, 388–396.

[10] Boring, E. G., 'Auditory theory with special reference to intensity, volume, and localization,' *Amer. J. Psychol.*, 1926, 37, 157–188.

not clear. He rests his claim on a psychological theory of inhibition which defines attention as predetermined, so that when the stimuli are too numerous for the range of attention, only that stimulus is effective for which the organism is in some way predetermined; and which holds that every mental process and presumably its neural correlate undergoes a process of culmination and decay.

Stewart [11] believes that while intensity differences at the ears are no doubt a factor in determining localization, such differences cannot account for the ability to localize a sound source, because the ratio of intensities required to produce displacement under experimental conditions is much greater than can result from the head-shadow in conditions of ordinary hearing. He maintains that the organs of hearing must respond to phase as such, and that complexity in the conditions of hearing gives phase a greater effectiveness.

Hecht [12] concludes that in 'stationary-sound-fields' such as are created by continuous sounds of very high frequency, intensity is the determining factor in localization; that phase is the effective agent in fields of very low frequency; and that time, phase, and intensity are all effective in fields of frequencies ranging between the very low and the very high. He describes localization as a 'capacity of the brain.'

Halverson [13] believes that phase, time, and intensity may be reducible to some common factor of auditory effectiveness, since phase-lead may be equivalent to the greater effectiveness secured by either greater intensity or priority of stimulation.

Restatement of the Theory.—Many other investigators have contributed to this aspect of auditory theory, but the above summary is sufficient to indicate the general theoretical trends. Some would have it that a single one of the physical factors predominates in determining localization under ordinary conditions. But, since all of these factors are approximately equally effective in determining localization under

[11] Stewart, G. W., 'Acoustics,' Iowa City, 1925.

[12] Hecht, H., 'Über die Lokalisation von Schallquellen,' *Naturwiss.*, 1922, 10, 107–113.

[13] Halverson, *op. cit.*, 1st.

experimental conditions; and, since each factor has its optimal effectiveness; and, since the conditions of normal hearing involve more than one of the factors in question, it would seem that this interpretation assumes too much. Others would reduce time, phase, and intensity to intensity or to some other common factor; but, since each of these is effective under conditions in which the others are present though equalized at the ears, this reduction, it seems, does not follow. Still others would limit the range of the effectiveness of each of the different factors to different frequency ranges, phase being the effective agent in ranges of low frequency, intensity in ranges of high frequency, while time, phase, and intensity, all three, are effective in the intermediate ranges. This, in the writer's opinion, is a more adequate interpretation of the facts; but the range of effectiveness in each case is perhaps too closely defined.

In this criticism of current theories of sound localization, the limitations have been set forth in each case: it remains to define a theory that meets the conditions.

In the light of the facts, it seems reasonable to assume that in the complex conditions of ordinary hearing, all three factors, *i.e.*, phase, time, and intensity, and possibly also mass, contribute to a difference-pattern that results in directional localization. Each factor contributes to the pattern to a varying degree, depending upon the magnitude of its difference in the different situations. Under ordinary conditions, except when the sound source is in the median plane, one ear is always nearer the source than the other, which means that there is always, under such conditions, a phase, a temporal, and an intensive difference at the ears. Under such conditions it seems unreasonable to assume that the organs of hearing analyze the stimuli and respond to a single factor. Phase may contribute more to the pattern under certain conditions than all the other factors; and intensity may be the main determining factor under other conditions, because the difference, in each case, is greater than the combined differences of the other factors. The same thing can be said of the temporal factor. And all of the factors

may contribute more equally in situations in which their differences are more nearly equal. Directional localization is a cortical function, an immediate, unanalyzable experience in the sense that the 'effects' of the different stimuli cannot be identified. The organs of hearing serve as mediating agencies for transmitting to the central nervous system the effects corresponding to the differences in the stimuli.

This 'difference-pattern' hypothesis may seem far-fetched in that it reduces a complex of stimuli to a pattern of a phenomenological order; but, under experimental conditions, observers cannot analyze the stimuli; they cannot tell whether it is a phase difference, or a temporal difference, or an intensive difference that conditions the localization which they perceive. In other words, any one of the physical factors may furnish a binaural difference-pattern that results in localization. Then, is it unnatural to assume that a complex of such factors will furnish such a difference-pattern?

According to this hypothesis, then, no single one of the physical factors in question predominates, in the absolute sense, in determining directional perception under ordinary conditions. Nor, are such factors reducible to one, except as they contribute to a common difference-pattern. The directional localization of a sound source, under the ordinary conditions of hearing, depends upon the configurational nature of the cortical effects that correspond to the physical 'difference-pattern' at the ears.

[MS. received May 17, 1928]

DISCUSSION

PURPOSIVE BEHAVIOR

In reading Mr. Kuo's article 'The Fundamental Error of the Concept of Purpose and the Trial and Error Fallacy'[1] I am particularly struck by three difficulties which he finds in the doctrine of purpose as an objective phenomenon. And I would like to state what might be the answers of us 'purposivists' to each of these difficulties.

1. The first of his difficulties which I note is that, as he says, a drive, tendency, purpose is something which we objective teleologists have to infer from behavior and not something we can directly observe in it. And, when we have inferred it, it seems to him that we describe it in the same terms as a mentalist uses although we deny that we use introspection.

Now, I can not be certain for the other members of the objectivistic teleological group[2] but it seems to me that we could answer this by declaring that the mere fact that the variable which we have called drive, tendency, purpose, has to be inferred from its effects and cannot be directly sensed need not mitigate against it. For most, and indeed in the last analysis perhaps all, of the concepts (variables) which figure in the equations of the natural sciences are so inferred. Such variables are never directly seen face to face. Space, time, number, velocity, heat, force, energy, electricity as used in physics are never immediately given in perception. They are all purely conceptual entities invented to give order, coherence, explanation to actual appearances. They are entities inferred from their effects. So why should not I be allowed to invent similarly inferred entities, to wit, those of behavior-drives or purposes?

Further, we would have to reply that our concept of purpose, or drive, is not, even though inferred, therefore mentalistic or introspectionistic. We do not describe or define these purposes in terms of their introspected 'feels.' Indeed I know that I, for one, am

[1] PSYCHOL. REV., 1928, 35, 414–433.

[2] I wish to acknowledge in this place my fundamental indebtedness to the other purposivists whom Mr. Kuo cites and in particular to Professors Perry and Woodworth. See especially Perry's article 'Docility and purposiveness,' PSYCHOL. REV., 1918, 25, 1–20. Also his 'General Theory of Value,' Longmans, Green & Co., 1926.

quite unable to identify such 'feels.' It may be that from introspection, so-called, I could tell you correctly and before otherwise behaving that I have a purpose, or it may be that I could not. (The times in which one is fooled by introspection as to his purposes seem quite as frequent as those in which he is not.) But this occasional ability to report correctly the mere presence of a purpose before that purpose appears in my other behavior is not introspectionism or mentalism in the technical sense. It conveys to you no different information about me from that which you would obtain if you waited for me to behave, or which you obtain about a rat when you wait for it to behave. It conveys nothing about the 'feel' of that purpose. You learn merely that acts leading to such and such respective goals are likely to be performed and persisted in, by me and by the rat.

But perhaps it will help to state this objective doctrine more positively. The essential character and definition of a behavior-purpose or drive for us purposivists is the fact of the *contingency* of an act upon its tendency to reach (or get from) a specific type of *end-object.* Where such a contingency exists the act in question is to be defined as having 'purposed'[3] that end. And what we mean by such a contingency is the fact of the learnability (or as Perry says, the *docility*) of the act with respect to its end. Whenever upon successive occasions, a given act will repeat itself, only if the getting to (or getting from) such and such a type of end-object is achieved by it, this act can be said to be contingent upon such an end. And it can be said to 'purpose' that end.

A purpose is a condition in the organism (analyzable presumably, if we knew enough, into physiology and then into physics and chemistry) whereby acts which lead to (or from) given types of end-situation persist and get learned, whereas those which fail in such gettings to (or from) drop out, get unlearned. This is a remarkable phenomenon. But it is not a supernatural or spiritualistic one. It is a perfectly objective feature which appears in behavior. And one of the chief tasks of the behaviorist is to note and record its appearances. To observe the range and distribution throughout the animal kingdom of such contingencies of acts upon their ends and the limitations and combinations of which such contingencies are capable, is one of the main jobs of the psychologist. He must, to be sure, explain such contingencies as soon as he can in physiological terms, but this need for a physiological explanation

[3] The terms purpose, drive, tendency, seem to me interchangeable.

does not mean that he can deny or ignore the contingencies themselves. And the further fact that introspecting organisms do not always have to wait to act in order to discover that their behavior is probably going to be in this way contingent—the fact, in short, that they can often correctly report purposes before those purposes appear in gross behavior is, of course, an even more remarkable phenomenon. It needs much study and explanation. But the existence even of this phenomenon is a testimony not to an essentially mentalistic character in purpose but merely to the complexities of the behavior-situation.

In short, our doctrine, to put it somewhat differently, is that behavior (except in the case of the simplest reflexes) is not governed by simple one to one stimulus-response connections.[4] It is governed rather by more or less complicated sets and patterns of adjustment which get set up within the organism. And in so far as these sets or adjustments cause only those acts to persist and to get learned which end in getting the organism to (or from) specific ends, these sets or adjustments constitute purposes.

2. Mr. Kuo's second difficulty seems to be that our doctrine cannot yet state the exact physiology which underlies such sets or adjustments. This is indeed true. But I find it an invitation to further investigation. I wonder indeed whether the alternative doctrine of relatively simple one to one stimulus-response connections has now any more acceptable a neurological picture at its service. I doubt if it does. Before Franz's and Lashley's results appeared, simple reflex-psychology had a doctrine of specific and insulated sensory-motor paths to fall back on. But now that this simple picture has been taken away, is it, I wonder, any better off as to neurology than is our purposive doctrine?

3. Mr. Kuo in his third difficulty seems, if I do not mistake him, to turn about face and to declare that our doctrine has a physiology but that it is a bad one. He refers, that is, to the hypothesis often advanced that the drive or tendency toward a goal can be conceived physiologically as the incipient going off of the response to be made to the goal when the latter has been reached. Thus Woodworth implies that it is an incipient state of the *consummatory* response which constitutes the drive which releases and controls the *preparatory* responses. Now while I hold no especial

[4] That is, what the Gestalt psychologists have labelled the Constancy Hypothesis, as they themselves have suggested (*e.g.* Koffka, *Psychol. Forsch.*, 1922, 2, 382–384) is as inadequate for stimulus response connections as it is for stimulus sense-quality connections.

brief for this specific hypothesis, I do not see anything essentially bad about it. It is perhaps as good a physiological guess as any other. But in any case, the main doctrine that there *are* guiding sets, tendencies, drives, whatever their physiology, does not, as I see it, have to stand or fall with this particular physiological addendum.

Let us see now what Mr. Kuo substitutes for our doctrine of drives. He writes as follows:

"I think it needs no argument to convince a keen observer that the only selective agency of behavior is stimulus. . . . What response the animal will make at a given moment depends upon the *prepotency* of the stimuli (intraorganic as well as external), *prepotency in both momentary and historical sense.* Suppose a rat has been kept without food for one or two days. The rhythmic contractions of the alimentary tracts may become the most prepotent stimulus which may last for a time and make the animal produce various restless movements. In this case, the purposive psychologists will insist that the animal in question is seeking for some goal (food), for, so they argue, as soon as food is given, the restless movements of the animal disappear. My reply is that these restless movements are no indication of goal-seeking on the part of the animal, they are merely the direct result of that prepotent stimulus we call contractions of the alimentary canal; they last as long as contractions of the alimentary canal remain prepotent, they disappear as soon as the contractions cease to be prepotent. They are just as specific and complete responses, as the actual eating of food; they have no reference to an end or to the future; for *they end with themselves* and their future is the shifting of prepotency from the contractions of the alimentary canal to some other stimulus." [5]

It appears from this quotation that the function which we purposivists have ascribed to drives or steers Mr. Kuo ascribes to what he calls the *prepotencies* of stimuli (particularly intraorganic stimuli). Thus, alimentary contractions are prepotent stimuli, which cause food to be eaten when it is present and which cause 'restless movements' when it is absent. But both responses, that of eating the food when it is present and that of 'restless movements' when it is absent, result, he says, from direct stimulus response connections. The 'restless movements' no more than the eating of food have any reference to the future. But it is here

[5] Kuo, Z. Y., 'The fundamental error of the concept of purpose and the trial and error fallacy,' PSYCHOL. REV., 1928, 35, 420–421. Italics mine.

that we would take exception. For it is here that such a doctrine, at least as far as it has yet been worked out, seems patently inadequate to the experimental facts. For, as I shall show in a moment, there are certain recent experiments which indicate that actually the restless movement responses *do* have a reference to the future. The character which they assume on later repetitions is contingent upon the ends achieved by them in earlier repetitions.

The first experiment of this sort which I wish to report is that of Blodgett. Blodgett [6] found that, if hungry rats are run daily through a maze but with no food at the end (they were fed only in their living cages some two hours later), their 'restless movements' in the maze are not of the same character as those of rats who are fed in the ordinary fashion after each run in a food-box at the end of the maze. The 'restless movements' of the latter or control group rather quickly ceased to include many blind entrances. They exhibited, that is, the usual learning curve. The 'restless movements' of the former group, on the other hand, continued to contain about as many blind-entrances as at first, *i.e.* their learning curve remained practically horizontal as long as no food was given. On the seventh day (when the control or reward group had practically learned the maze), food was suddenly introduced into the end-box of the maze for this hitherto unfed group. The result was that on the eighth and ninth days there appeared an enormous drop in the number of their blind entrances. In fact, by the ninth day these animals were making practically as few errors as the control, or continuously fed, group. In a word, the 'restless movements' in the maze of the animals in this experimental group were of one character as long as no food was found at the end of the maze, but they took on an entirely different character almost immediately after finding such food. That is, such 'restless movements' in the maze do have a reference to the future. They are contingent upon what results from them. They change their character as that result changes.

This fact of the contingency of maze-responses upon the results at the end of the maze has been further investigated and elaborated

[6] Blodgett, H. C., 'The relation of reward to animal learning,' Ph.D. thesis, University of California, 1925, to be published shortly in the *J. Comp. Psychol.* It should be noted that the phenomena here subjected to a careful investigation by Blodgett had previously been reported, though in a sketchy fashion, by both Simmons (*Comp. Psychol. Monog.*, 1924, **2**, No. 7) and Szymanski (*Pflüger's Arch. f. d. ges. Physiol.*, 1918, **171**, 374).

by Miss Adams.[7] She found that blind entrances will suddenly drop out not only as in Blodgett's experiment, when rats previously unfed in the maze suddenly find food, but also when they suddenly find a familiar eating box though containing no food in it. This familiar eating box was a Discrimination Box in which they were accustomed to being run every afternoon, some two hours after the maze running. Upon suddenly finding this Discrimination Box at the end of the maze they, in the next few trials, brought their learning curve in the maze down to the level of that of a control group which had found the Discrimination Box but *with food in it* throughout the entire maze experiment. Significantly enough, however, this drop in the learning curve resulting from finding the Discrimination Box but no food in it, was not permanent. After some half dozen more trials the animals so treated discovered the 'sell,' and reverted to as many blind entrances as they had been making before finding this empty Discrimination Box.

Finally Elliott[8] has investigated still further this contingency of the movements in the maze upon what is found at the end of the maze. He fed rats during the first part of the learning curve with one sort of food and then towards the end of learning switched them to a new sort of food. On the days succeeding this switch these animals began making significantly more errors than did a control group who had been fed with this second sort of food from the very beginning.

Summing together these results of Blodgett, Miss Adams, and Elliott,[9] we may say (1) that the 'restless movements' in the maze will change in character when a switch is made from no food at the end of the maze to food; (2) that they will likewise change when a switch is made from no food to a customary 'sign' for food until further experience causes this sign to lose its signification in this specific situation; and (3) that they will change their character when a switch is made from a previously given customary food to a new unfamiliar food.

From these facts I would conclude that the sort of movements

[7] Adams, K., 'The reward value of a conditioned stimulus,' Ph.D. thesis, University of California, 1928. To appear in the University of California Publications in Psychology.

[8] Elliott, M. H., 'The effect of change of reward on the maze performance of rats,' M.A. thesis, University of California, 1928. To appear in the University of California Publications in Psychology.

[9] Let me take this occasion to acknowledge my extreme and fundamental indebtedness to the ideas as well as to the experiments of each of these three investigators.

released by hunger contractions are contingent, not only upon these hunger contractions themselves and upon the external stimuli coming from the maze alleys *per se*, but also upon the characters of the end-situation which the animals upon preceding trials have found to result. And it is such facts which, as I see it, the objective purposivists have tried to allow for and to schematize by the concept of determining propensities, behavior-drives, steers, tendencies, purposes. For such a concept permits a fairly ready description and summary of the fact that individual behavior acts are guided and determined not only by present stimuli, internal and external, but also by the ends which have resulted from the responses to these stimuli upon preceding occasions.

One last word. It is perhaps not to be expected or to be hoped that the above statements will satisfy Mr. Kuo. And my guess is that he will be inclined to reply that such facts as those presented by Blodgett, Miss Adams, and Elliott can be taken care of in the terms of the doctrine of prepotent stimulus. He will, I venture to suppose, feel that, if one properly includes under the notions of prepotent stimuli, not only the stimuli then and there sensorially present but also the stimuli met by the rat at the end of the maze upon the preceding trials, such an account will be as adequate as ours. He will say that the entrance of particular blinds is to be described as determined not only by the alimentary contractions then present and by the stimuli coming from these immediate maze parts but also by the stimuli received at the end box in preceding trials. But my answer would be that if he says all this and amplifies it in such detail so that it covers all the possible complications for results such as those cited,[10] then I for one can raise no objection.

But actually I do not believe that in the present state of our knowledge of stimulus response connections he can be successful in saying all this. Or, in other words, I do not believe that the concept of prepotent stimulus can be as helpful for immediate further experimentation as can that of objective behavior-purposes.

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[MS. received May 22, 1928]

[10] Note particularly the problems raised by Miss Adams' results.

SOME QUERIES AS TO KUO'S DOCTRINE OF PASSIVITY

In his vivacious and interesting paper entitled 'Purpose and the Trial and Error Fallacy' (PSYCHOL. REV., 1928, **35**, pp. 414–433) Professor Kuo repeatedly asserts that animal and human behavior is always passive. It is in every case completely determined by the stimulus. *The animal* decides nothing and contributes nothing. Neither animal nor human being has any *character* with which the psychologist is obliged to reckon.

The author seems to take a mischievous delight in shocking all the 'old fogies' of psychology, philosophy, and religion. His view, he assures us, is absolutely necessary, if animal and human behavior are to be brought within the scope of science. And once it *is* accepted, he exuberantly assures us, the whole of psychology prior to the epoch-making works of Watson and Weiss must be consigned to the waste basket. Probably he would add all philosophy prior to Durant's.

But when hydrogen and oxygen combine in their characteristic fashion, is the chemical reaction determined solely by the hydrogen? Is the oxygen 'passive' in Dr. Kuo's sense? And when the earth attracts a stone, is the force between the two bodies a function of the earth's mass alone? Is it not rather a function of the masses of both the earth *and* the stone? Does not the same quality hold in every instance of magnetic or electronic attractions or repulsions? And, in general terms, is not every action an *inter*action? Do not *two* factors, at least, enter into every chemical, physical or mechanical process? And must not any description of such a process take account of all the factors? And shall we consider electrons or atoms genuine factors but deny that organisms are? To what shall we attribute such degeneracy in organisms?

It is quite possible that I mistake the author's meaning. But it seems to me that his description of animal behavior renders it unique and without parallels in any department of science. His 'passive' organisms, far from being analogous to other entities with which science deals, seem to be creatures of an entirely different order. It is entirely plausible that an entirely new science will have to be devised to deal with them—like that taught in the Colleges of Unreason in Butler's delectable land of Erehwon. This, however, supposes that these new entities are to be taken seriously.

It would be wrong to close this little group of queries without

a word of appreciation for Professor Kuo's able article. He has mercilessly, but I think justly, criticized such ingenious devisers of circumlocutions as Perry, Peterson, and Tolman. He has made the argument respectable that, if effects are to function in animal behavior, their functioning can only be described in *mentalistic* terms. Mentalists have every reason to be encouraged.

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[MS. received October 8, 1928]

A REPLY TO DR. KUO

With reference to Dr. Kuo's recent article, 'The Fundamental Error of the Concept of Purpose and the Trial and Error Fallacy,' I wish to say the following. Dr. Kuo presents the thesis that the concept of purposive behavior is out of harmony with behavioristic premises of the kind he holds. I take pleasure in expressing complete agreement at this point. But purposive behavior occurs as an observable fact. We do see men attempting to recover wind-blown hats. Sometimes, at least, such observations are as 'objective,' as free from *observable* inference, as Dr. Kuo's most 'objective' observations of the movements of the totem animal of behaviorism. To be sure, either Dr. Kuo or any one else may have been mistaken in any given instance, but accuracy of observation is demonstrated by subsequent verification, not by 'objectivity' as defined by the behaviorist. Dr. Kuo would hold that 'attempting to recover a hat' is merely a convenient summary of physiological facts the exact nature of which we do not know. That would seem to depend upon the physiologist; there are physiologists who cannot do without the concept of purposive activity and who, accordingly, have concepts for summarizing such facts. But physics and chemistry of the present day have no *concepts* in which they can express the fact that 'the man is attempting to recover his hat.' This inability is *not* due to ignorance of facts, but to the very fact which Dr. Kuo stresses that concepts involving purpose are foreign to these sciences. Accordingly it is difficult to see whence Dr. Kuo and his confrères derive authority for banishing from science observable facts,—and concepts which denote them.

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